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Original Communications.

PROPHYLACTIC HYGIENE.

LECTURE IX.—ON PASTEURIAN VACCINATIONS.*

By Professor Dujardin-Beaumetz, Paris, France.†

GENTLEMEN:—Nearly ten years ago (February 10, 1880) Pasteur announced to the Academy of Sciences that the virulence of the cultures of the microbe of chicken cholera could be made to undergo a progressive attenuation, and that the inoculation in the fowl of this attenuated virus confers immunity against any subsequent attack of the disease; he laid down this law, moreover, that each attenuated virus constitutes a vaccine for the virus less attenuated.‡

The year following (March 21, 1881) he made known in his own name and that of his collaborators, Chamberland and Roux, that it was also possible by special processes to attenuate the virus of charbon (splenic fever,

* Advance sheets of a lecture delivered in Cochin Hospital, and translated for this journal by E. P. Hurd, M.D.

† Member of the Academy of Medicine, Physician to Cochin Hospital, etc., Paris, France.

‡ Pasteur, "On Virulent Diseases, and Chicken Cholera in Particular" (*Acad. de Méd.*, February 10, 1880); "On Chicken Cholera, and Exemption from Relapses, etc." (*Acad. de Méd.*, April 27, 1880); "Attenuation of the Virus of Chicken Cholera" (*Acad. des Sciences*, October 26, 1886).

wool-sorters' disease), and render animals refractory thereto; and the 5th of May of that same year took place that celebrated experiment at Pouilly-le-Fort, which demonstrated the reality of the facts advanced by Pasteur.*

The very same year (1881) Pasteur commenced his studies on canine rabies;† he showed that by similar processes the development of that disease might be effectually opposed in the dog and in man; and on the 6th of July, 1881, at eight o'clock in the evening, in the presence of Vulpian and Grancher, he applied this method to a boy by the name of Joseph Meister.

Such are the steps in the history of attenuated virus and resulting vaccinations,—a history which does such lasting honor to our illustrious fellow-countryman, whose name will be henceforth associated with this grand discovery,—a discovery which places Pasteur above Jenner, for, as I said in my first lecture, if the one made a discovery of genius, the other has created a method of genius.

But there is another man whose name ought not to be forgotten in connection with this discovery of attenuated virus. I refer to Toussaint. On the 12th of July, 1880, Toussaint communicated to the Academy of Sciences a memoir, in which he endeavored to show that by filtration, or by elevation of the temperature, one might create an attenuated virus, which would confer on animals immunity.‡ Toussaint maintained that the *bacillus anthracis* produced its infectious effects only by its soluble products, possibly certain alkaloids secreted by these micro-organisms. He was then the first to make known the possibility of conferring on animals immunity by the toxines secreted by the microbes,—a process of vaccination to which has since been given the name of chemical vaccines. He was the first, also, to show the influence of heat in attenuating the virulent properties of the anthrax-bacillus. I propose to take up to-day the principal points suggested by this great question of Pasteurian vaccinations.

When we take a general survey of all the processes which experimental physiology has at command for the obtention of these attenuated viruses, which are true vaccines, we see that they may be ranged in three great classes:

In the first, the attenuation results from modifications, impressed upon the vitality of the microbe. In the second, we no longer act directly on the micro-organism, but we obtain the attenuation by diminishing the number of the micro-organisms introduced, by modifying the mode of introduction, or by making the microbes pass through different organisms. Lastly, in a third group, we no longer direct our efforts to the micro-organism itself, but to the leucomaïne which it secretes, and here it is the chemical element which constitutes the preservative vaccine. Let us examine each of these groups.

We may by various means, by physical or chemical agents, modify the vitality of bacteria, and thus transform pathogenic microbes into saprogenous microbes, according to the expression of Chauveau.§ This transformation has given rise to important recent communications. Some believe with Bouchard‖ that the microbe may secrete different ptomaïnes, some virulent, others vaccinal, and that we may succeed by physical and chemical agents in causing the first to disappear while preserving the second; such is not the opinion of Chauveau, who thinks that the vaccinal properties are due to diminution of the virulence proper to the pathogenic microbe.¶

It must be borne in mind that we have not yet a physiological explanation of the immunity conferred by these vaccines, and we may yet have to wait a long time before science will have said the last word on this vexed question: this is a subject which I have already touched upon under the head of infections and poisonings. We come now to the practical question how we may attenuate the viruses of infection by means of physical and chemical agents. We will begin, if you please, with the first.

The physical agencies which we may employ are heat, light, and desiccation.

* Pasteur, Chamberland, and Roux, "On the Attenuation of Virus, and its Return to Virulence" (*Acad. des Sciences*, February 28, 1881); "The Vaccine of Charbon" (*Acad. des Sciences*, March 21, 1881).

† Pasteur, Chamberland, Roux, and Thuillier, *Comptes-Rendus de l'Acad. des Sciences*, May 30, 1881, and December, 1882.

‡ Toussaint, "On Immunity from Charbon, conferred by Preventive Inoculation" (*Acad. des Sciences*, July 12, 1880, and July 18, 1881).

§ "On the Vaccinal Properties of Microbes before Pathogenic, transformed into Microbes of Saprogenous Appearance" (Chauveau in *Archives de Médecine Experimentale*, etc., March, 1889, p. 161).

‖ Bouchard, "On the Attenuation of Virus" (*Acad. des Sciences*, February 25, 1889).

¶ Chauveau, "On Microbes previously Pathogenic, which, to all Appearance have preserved only the Property of Vegetating Outside of the Living Organism. Can they Recover their Primitive Infectious Properties?" (*Acad. des Sciences*, March 4, 1889).

Heat was one of the first agencies put in use for the attenuation of virus, and Toussaint,* as I have told you, was the first (on July 12, 1880) to call attention to this means of attenuating anthracoid blood, and to provide veterinary medicine with a real vaccine for charbon. This process consists in heating anthracoid blood, thoroughly defibrinated, to 55° C. (131° F.) for ten minutes, and in practising with three cubic centimetres of this blood subcutaneous injections. Toussaint, in operating on a sheep, obtained immunity by a single inoculation, and in three other sheep by two inoculations.

But it is to Pasteur that the honor belongs of having established on definite bases this attenuation of virus, and of having shown how the *bacillus anthracis* becomes modified under the influence of heat; already the previous year, while studying chicken cholera, he had shown that it was easy to attenuate cultures of the microbe of this disease by a process of *aging* (*vieillissement*), and that this loss of virulence went on gradually to complete inactivity. The organisms whose virulent properties have been thus attenuated transmit this attenuation to their descendants, so that you may at will constitute races of microbes of almost any degree of attenuation. As I shall tell you presently when I come to speak of oxygen, it is to the action of this agent and to that of light that we must attribute this attenuation. It is, then, under the guidance of these first researches that Pasteur undertook the attenuation of anthracoid virus.†

Pasteur and his collaborators, Chamberland and Roux, showed that at the temperature of 42° C. (107° F.) cultures of *bacillus anthracis* give rise to abundant tufts of mycelium (see THERAPEUTIC GAZETTE, October 15, 1888, page 652, Fig. 3), in which you no longer observe the development of spores. Above 45° C. (113° F.) the cultures are arrested. It is this mycelium of the bacillus anthracis, deprived of spores, which is susceptible of undergoing the action of oxygen and losing its virulent properties. We have here, as you see, a double action: the action of heat which modifies the culture of the bacillus anthracis and makes of it a species which develops without sporulation, and the action of oxygen which attenuates the virulence of the mycelia.

We thus obtain two vaccines: the one is a first vaccine which results from the sojourn of a culture of the bacillus deprived of spores in a stove where it is exposed to a heat of 45° C. (113° F.) for fifteen to twenty days; this vaccine does not kill hares and guinea-pigs, but it kills mice. The second vaccine is produced by a similar culture, which is allowed to remain in the stove only ten or twelve days; this vaccine kills mice and guinea-pigs, and renders the adult hare sick without killing it.

It was with such vaccines that on May 5, 1881, was made the celebrated experiment of Pouilly-le-Fort which you should always remember, for it was in consequence of this memorable experiment that this subject of attenuated virus first passed from the domain of the laboratory to that of practice. The Society of Agriculture of Seine and Marne, presided over by the Baron de la Rochette, decided that a test experiment should be made at a farm situated near Melun at Pouilly-le-Fort.‡ This experiment was begun May 5, 1881. Twenty-four sheep, a goat, and six cows were inoculated that day with the first virus; twelve days after a second inoculation was made with the second attenuated virus. On the 31st of May all these animals were inoculated with an extremely virulent virus, as well as thirty-two other animals that were to serve as tests.

The next day every one who participated in this experiment was on the ground to witness the result, and you may imagine the feelings of those present when they saw in the yard where were kept the animals which had not been inoculated, twenty-one dead sheep, and all the cows sick, while, on the other hand, all the animals that had been vaccinated resisted the infection.

The experiment was conclusive, and henceforth it was a settled fact that it was possible, by the method indicated by Pasteur, to protect our flocks from the pestilence which had been making such havoc among them; and I shall show you, when I come to speak of the practical consequences of the Pasteurian vaccinations, the results which we have attained to-day.

Light, as I have said, is one of the agents

* Toussaint, "On Immunity from Charbon acquired by Preventive Inoculations" (*Acad. des Sciences*, July 12, 1880).

† Pasteur, "On Chicken Cholera," "Study respecting the Conditions of Immunity," etc. (*Acad. des Sciences*, April 27, 1880); "On the Attenuation of the Virus of Chicken Cholera" (*Acad. des Sciences*, October 26, 1880).

‡ Pasteur, Chamberland, and Roux, "Report of Experiments made at Pouilly-le-Fort on Anthracoid Vaccination" (*Acad. des Sciences*, June 13, 1881).

of attenuation of virus, and in proof of this I need only appeal to the labors of Arloing. Arloing operated on the *bacillus anthracis;* he first tried gaslight, then sunlight, and showed the rapid destructive action of the latter, even on the spores of the bacillus. This attenuating influence is not due to the calorific rays, nor the luminous rays, but to the solar light in its entirety.*

This rapid action of the solar light on the spores of the *bacillus anthracis* has given rise to some interesting discussions. When I spoke to you of pathogenic microbes, I told you how resistant are the spores of this bacillus to our ordinary agents of destruction. The results to which Arloing arrived were in absolute contradiction to this statement. Hence we have seen doubts expressed as to the value of those experiments, and Nocard on the one hand, and Straus† on the other, take the ground that the solar rays act not on the spores but on the nascent mycelium, stimulating the germination, but rapidly destroying the new proliferation.

Arloing has since repeated the experiments of Straus, and has reaffirmed his first statements.‡ He believes that the solar light acts equally on the spores and on the mycelium, and even the mycelium is the more resistant especially when it is very young. Light is, then, one of the most powerful agents of destruction of the microbes, and enables us to obtain a rapid attenuation of anthracoid virus.

Desiccation is also a means of attenuation of virus, and this method, joined to that of heat, has been employed on two kinds of virus,—that of symptomatic charbon and that of rabies.

In my lecture on the pathogenic microbes, I told you that there are two species of charbon,—the one which attacks our sheep, the deadly anthrax, whose pathogenic microbe is the *bacillus anthracis;* the other is the symptomatic charbon, which makes such ravages among cattle, and is known by the name of *mountain plague* (*mal de montagnes*); its pathogenic microbe is the *bacterium Chauvei.* These two diseases are absolutely distinct, one might even say opposite.

Arloing, Cornevin, and Thomas, who have made a complete study of symptomatic charbon, and who have examined all the methods of attenuation of the *bacterium Chauvei*, have chiefly utilized in their method the employment of heat and of desiccation, and this is their mode of procedure. They desiccate in a current of dry air the anthracoid muscular pulp; they reduce to a powder the mass thus desiccated, moisten it, and subject the whole to a temperature of 100° to 104° C. (212° to 220° F.). At the end of seven hours they obtain a virus which is very much attenuated, and which may serve for a first vaccine. To obtain the second vaccine, they still make use of the dry pulp, and subject it for six hours to a heat of from 85° to 90° C. (185° to 192° F.). But it is in the attenuation of the rabic virus that the action of light, of temperature, and desiccation is especially brought into usage.

When Pasteur, in 1881, showed that it was in the nervous system that the virus of rabies localizes itself, thus transforming into scientific reality the hypothesis advanced in 1879 by Duboué, science was henceforth in possession of a method by which multiple and precise experimental researches might be made on canine rabies.§

Basing himself on the period of incubation, Pasteur established the principle that we may augment or diminish the virulence of rabies by causing the disease to pass through different organisms, and he also showed that it was possible to obtain for each animal a fixed virus. Street rabies, communicated from hare to hare, gives at the end of a certain time a virus which, by intracranial inoculation, causes the same disease to appear in seven days.

It is this fixed virus which Pasteur has attenuated, and which serves as the basis of his vaccinations. To obtain this attenuation, he put in operation processes which had already served him in other attenuations. As thus

* Arloing, "Influence of Light on the Vegetation and on the Pathogenic Properties of the Bacillus Anthracis" (*Acad. des Sciences*, February 9, 1885); "Influence of Sunlight on the Vegetation of the Spores of Bacillus Anthracis" (Ibid., August 24, 1885); "Influence of Light on the Vegetation and Virulence of Bacillus Anthracis" (*Lyon Médical*, February 7, 14, and 21, 1886); "Influence of White Light and its Constituent Rays on the Development and the Properties of the Bacillus Anthracis" (*Archives de Physiologie*, 1886); "Destruction of the Spores of the Bacillus Anthracis by Solar Light" (*Acad. des Sciences*, March 7, 1887).

† Straus, "Note on the Action of the Solar Light on the Spores of Bacillus Anthracis" (*Soc. de Biol.*, November 6, 1886).

‡ Arloing, "Destruction of the Spores of Bacillus Anthracis by Solar Light" (*Acad. des Sciences*, March 7, 1887).

§ Pasteur, Roux, Chamberland, and Thuillier (*Acad. des Sciences*, May 30, 1881, and December 11, 1882). Duboué, "On the Pathological Physiology and Rational Treatment of Rabies." Paris, 1879. "On the Progress accomplished in the Knowledge of Canine Rabies, and on the Part which Belongs to the Nervous Theory," 1887.

far it has been impossible to isolate the pathogenic microbe of rabies and to develop it in culture-broths, he chose the spinal cord as the most suitable infecting material to subject to the action of heat and of desiccation.

The method consists in suspending in glass jars, which are exposed to the rays of the sun, the spinal cords of hares which have succumbed to rabies, and these cords are there kept at a constant temperature of 20° C. (68° F.), and the air which penetrates these jars is desiccated by the agency of caustic potash. The virulence of these cords becomes gradually attenuated, so that at the end of seven days they can no longer communicate rabies. This attenuation, at first very feeble during the first two days, becomes more pronounced on the third day, then goes on increasing more and more.

Temperature has a very notable influence on the attenuation of virus. Gamaleia has shown that it suffices to elevate the temperature from 3° C. to 23° C. to cause the spinal cord to lose all its virulence in five days. Helman, of St. Petersburg, affirms that in twenty-four hours this virulence is lost, if you raise the temperature to 35° C. (95° F.).

Thanks to this combined action of dry air, of heat, of light, and of time, Pasteur was now in possession of a scale of more or less attenuated viruses, proceeding from the most inoffensive to the most active. It is this graduated scale which he employs in effecting his antirabic vaccinations, going gradually every day from the weakest virus to that which is most powerful. I cannot here enter further into this great question of antirabic vaccinations, and must refer you to the numerous treatises written on this subject, and especially to the work of Suzor.*

In the operation of all these physical agencies for the attenuation of virus, heat, light, desiccation, there exists an always constant factor,—namely, oxygen. In fact, from the time of his first researches on the attenuation of virus, Pasteur had shown that in the attenuation of the virus of chicken cholera, oxygen was the really efficacious agent, and the experiment which he performed to determine this is very demonstrative. He placed in two flasks some cultures of the micro-organisms of chicken-cholera, cultures made under similar conditions and in the same nutritive medium; he sealed hermetically the first flask, and plugged the second with cotton wadding. The culture in the flask hermetically sealed did not undergo attenuation, while that in the other flask, exposed to the free action of the air, became remarkably attenuated. But Chauveau is the authority who has most thoroughly studied the action of oxygen on the attenuation of virus, and he has made this the subject of several very interesting memoirs.†

These researches were made from 1883 to 1885, and pertained exclusively to the attenuation of anthracoid virus. He endeavored first to ascertain which was the most active agent, heat or oxygen. He proved that there are conditions in which heat and air favor, hinder, or prevent, the development of the *bacillus anthracis*. To the first of these conditions he gave the name *eugenesic;* to the second, the name of *dysgenesic;* to the third, that of *agenesic*.

He showed that at the agenesic and dysgenesic temperatures oxygen plays no part, at least in elevated temperatures. It is not so with low temperatures, and the presence of oxygen hastens the loss of virulence and the loss of the property of proliferation. But when you augment the pressure of oxygen, the action is much more energetic, and this is how Chauveau demonstrates this fact. He places a culture of *bacillus anthracis* in a recipient sufficiently resistant to withstand the pressure of eight atmospheres; Chauveau uses atmospheric air rather than oxygen. The temperature of the medium is maintained at 35° C. (95° F.). At the end of three weeks there is an attenuation of this culture, but it is still feeble, as it causes the death of half the sheep inoculated with it.

But if you make with this first culture already attenuated, another culture in the same conditions and under the same pressure, you obtain a virus more attenuated still, and you can easily succeed, by thus renewing these cultures, in making them undergo successive attenuations. The latter pertain not only to the virulent properties of the cultures, but also to their reproductive properties, and, as in Pasteur's method, each of these cultures thus attenuated is the starting-point of a

* Suzor, "A Practical Exposition of the Treatment of Rabies by the Method of Pasteur." Paris, 1888.

† Chauveau, "On the Rôle of the Oxygen of the Air in the Attenuation of Virulent Matters by the Action of Heat" (*Acad. des Sciences*, March 12, 1883); "On the respective Rôle of Oxygen and of Heat in the Attenuation of Anthracoid Virus by the Method of Pasteur" (ibid., May 21, 1883); "On the Nature of the Transformations which the Blood of Splenic Fever undergoes by Culture in Compressed Oxygen" (ibid., July 13, 1885).

species of microbe whose vegetation and proliferation are henceforth fixed.

Chauveau affirms, even, that his process is much superior to that of attenuation by heat. Hence he has substituted in practice his method for that heretofore employed. This superiority results, he thinks, from the resistance which the cultures attenuated by compressed air present to the ordinary causes of age and decay. These cultures preserve their vaccinal properties for several months, and this period is the longer the more pronounced the attenuation. In the case of the vaccinal virus employed for sheep, this conservation holds good for three months.

By the side of oxygen we would place all chemical substances capable of modifying the vitality of these microbes, and it is an interesting fact that Chamberland and Roux have shown that most antiseptic substances have the property of attenuating the *bacillus anthracis*. Here are some of the facts brought out by their researches:

When we employ phenic acid in modifying culture-broths, we observe that in the strength of 1 per 800 it does not sensibly impede the development of *bacillus anthracis*, but, on the other hand, the proportion of 1 per 400 destroys all vegetation in forty-eight hours. But if we take the mean of these figures and add to a culture-broth of 35° C. (95° F.) carbolic acid, in the proportion of $\frac{1}{600}$, we obtain a culture deprived of spores and sufficiently attenuated.

Chamberland and Roux also showed that the culture-broth has a real importance in these cases, and that if, for instance, we substitute distilled water for the culture-broth, this same proportion of 1 per 600 of phenic acid entails the rapid death of the *bacillus anthracis*, and it is necessary to lower the proportion to 1 per 900 in order to obtain, in distilled water, the same attenuation that 1 per 600 produces in the ordinary broth.*

Arloing, Cornevin, and Thomas have, in their turn, studied with great care the action of antiseptics on the attenuation of the *bacterium Chauvei*. They have brought out this important and interesting fact that antiseptics act differently on each of the microbes which we wish to attenuate, and that it is impossible *a priori* to establish a law applicable to the attenuation of the different micro-organisms, and this is their conclusion: "It is possible, by properly grading the strength of certain antiseptic solutions and determining the proportions according to which these antiseptic substances should be associated and the duration of the contact, to prepare vaccines by the aid of which we may succeed in practising prophylactic inoculations."

To the long list of antiseptic substances experimented with by Arloing, Cornevin, and Thomas for the attenuation of symptomatic charbon, Nocard and Mollereau have added oxygenated water.† Gamaleia has applied to the attenuation of anthracoid virus the bichromate of potash, and it is thus that the vaccination for charbon has been practised in Russia.‡

Maximovitsch§ has studied the action of the naphthols on this same virus, and his experiments have brought to light this curious fact, that the naphthol α in the proportion of .3 per 1000 and the naphthol β in the proportion of .6 per 1000 destroy the vegetation of the *bacillus anthracis* in from fifteen to twenty minutes; but in order to destroy all virulence, five or six days of contact with the same solutions are needed. Hence, then, the vegetation of this bacillus and its virulence are at different points of the scale.

Maximovitsch has also caused these naphthols to act upon the pyocyanic bacillus, and he has shown, as in the case of the anthracoid virus, that it is necessary to distinguish in their antiseptic action the effects on vegetation from those on virulence. More recently, Grancher and Chautard have shown that if hydrofluoric acid does not destroy the bacillus of tuberculosis, it attenuates its effects; they have not, however, constituted a veritable vaccine with these bacilli.‖

Lastly, we ought to include among these facts the recent experiments of Manfredi,¶ who, in adding fatty matters to the culture

* Chamberland and Roux, "On the Attenuation of the Virulence of the Anthracoid Bacteria under the Influence of Antiseptic Substances" (*Acad. des Sciences*, April 9, 1883); "On the Attenuation of the Anthracoid Bacteria and its Germs under the Influence of Antiseptic Substances" (ibid., May 14, 1883).

† Nocard and Mollereau, "On the Employment of Oxygenated Water as a Means of Attenuation of Certain Viruses" (*Bull. de l'Académie de Médecine*, January 2, 1883).

‡ Gamaleia, "A Study of Anthracoid Vaccination" (*Annales de l'Institut Pasteur*, 1888, No. 10).

§ Maximovitsch, "New Researches in the Naphthols" (*Acad. des Sciences*, May 14, 1888).

‖ Grancher and Chautard, "Influence of the Vapors of Hydrofluoric Acid on the Bacilli of Tuberculosis" (*Annales de l'Institut Pasteur*, 1888, No. 5).

¶ Manfredi, "On Excess of Fat in the Alimentation of Pathogenic Micro-Organisms as a Cause of the Attenuation of their Virulence" (*Giorn. internez del scienze Mediche*, 1887).

media of *bacillus anthracis*, noticed that these fatty matters modified the culture and produced an attenuated virus. If there are chemical substances which attenuate virus, there are others, on the contrary, which exalt the virulent properties of virus, and I can give you a good instance of this in the effects of lactic acid on the virus of symptomatic charbon. Arloing and Cornevin have shown that you double its virulence by putting it in contact with one-fifth part of lactic acid. Viruses attenuated by other methods regain their first virulence when you bring them into contact with lactic acid. We have here an important fact, which shows that we may find in a same series of bodies substances which augment or enfeeble the virulence of the pathogenic microbes.

I pass now to the second group of means which may be put in operation for the attenuation of virus.

Here no attack is made on the vitality of the pathogenic microbe, but the attenuation is obtained either by introducing a very limited number of micro-organisms into the economy, or by modifying the mode of introduction, or in causing the microbe to pass through different organisms.

When I spoke of immunity, I mentioned the experiments of Chauveau, which showed that it was possible by introducing a very small number of the *bacillus anthracis* into animals to confer on them immunity. I shall not return to this point, and shall refer you to the very important memoir which Chauveau has just published, "On the Vaccinal Properties of Attenuated Virus."*

But I desire to dwell a little longer on the immunity acquired by the part of the body where the inoculation is practised. I shall choose my examples in the attenuation of the virus of symptomatic charbon and from canine rabies.

Arloing, Cornevin, and Thomas have shown that the mode of introduction of the *bacterium Chauvei* modifies considerably the effects of this microbe. Thus, when you inject this microbe into the blood, you produce discomfort and uneasiness for two or three days, but the animal does not succumb, and this injection confers immunity. This was, indeed, the first process of vaccination counselled by experimenters.

But as this process often gives rise to accidents, another method has been substituted, which consists in inoculating animals at the extremity of the tail, and this method is still employed to-day.

As for canine rabies, we find similar facts in connection with this malady, and we can, according to the mode of introduction, augment or attenuate the virulence. While the incubation is long and uncertain when the virus is inoculated under the skin, this incubation diminishes considerably when the inoculation is made directly into the brain. To conform to objections which have been made to this method of intracranial inoculation, some have recourse to-day to a process quite as rapid, and which produces no traumatism,—namely, the inoculation of the virulent matter into the anterior chamber of the eye, according to Gibier's process.

Such are the means for augmenting the virulence of rabies by the mode of introduction; but we may also attenuate its effects by injecting the virus into the blood, and a certain immunity may be obtaind, as Galtier has shown, by injecting into the veins of animals (as sheep and goats) the saliva of a mad dog.†

When you cause a virulent disease to pass from one organism to another, you either exalt or diminish its virulent properties, and you may be able by taking this altered virus from animals to obtain a preservative vaccine.

We come now to the third group which we have established,—namely, to the methods of vaccination,—where we employ not the microbe itself, but the culture liquids in which it is developed.

As I told you at the beginning of this lecture, it is to Toussaint that we are indebted for the first notion of this mode of vaccination.‡ In his communications made in 1880 and 1881 to the Academy of Sciences, Toussaint stated that he had obtained immunity from charbon by injecting in animals blood serum thus prepared. He took anthracoid blood, defibrinated it by whipping, then filtered it through ten or twelve thicknesses of filtering-paper; this liquid was thus deprived of bacilli, and the results obtained by injecting it gave support to the hypothesis put forth by him, that the *bacillus anthracis* produces its noxious effects, not by its own pres-

* *Archives de Médecine Experimentale*, p. 161, March 1, 1889.

† Galtier, *Compt.-Rendus de l'Acad. des Sciences*, August 1, 1881.

‡ Toussaint, "On Immunity from Charbon acquired by Preventive Inoculations" (*Acad. des Sciences*, July 12, 1880); "On Certain Points Relative to Immunity from Charbon" (ibid., July 18, 1881).

ence, but by the toxic products which it secretes. Unfortunately, as Pasteur showed, this filtration was incomplete, and let a certain number of bacteria pass through, so that the whole subject remained in the state of conjecture. The same hypothesis had already been advanced by Pasteur, who showed in 1880 the possibility of explaining the immunity by assuming the action of certain products secreted by the microbes. Chauveau, the same year, accepted the same hypothesis.

All these experimental researches were destined to be taken up again in 1887. But I ought to mention in this connection a work which has not been cited in the very complete history which Bouchard has given of this subject in his fine treatise "On the Therapeutics of Infectious Diseases."* This brochure, which is anonymous, appeared in 1886, and bore the title, "The Immunity conferred by Leucomaïnes."

The writer, who signs himself E. G. B., after having criticised all the works before published on attenuated virus, vaccines, and immunity, comes to the conclusion that it is the leucomaïnes secreted by the pathogenic microbe which, by entering the organism, confer immunity, whether this be owing to the penetration of the economy by the microbe, or to the introduction for vaccinal purposes of the leucomaïnes in the filtered products of the culture-broth.

The unknown writer, however, takes the part of a critic, not of an experimenter. But we shall see that from 1887 onward, experiments in this direction became more and more numerous and demonstrative, and that they establish the reality of vaccination by soluble matters.

In January 27, 1887, Pasteur, in a letter addressed to Duclaux, and published in the first number of the *Annales de l'Institut Pasteur*, under the head of "Canine Madness," admits that in the rabic rhachidian bulb (or spinal marrow) there are two things,—the virus and the vaccinal matter, thus separating the virulent from the vaccinal properties.

The same year, in the month of September, Salmon and Smith reported to the Medical Congress at Washington the results which they had attained, and showed that culture-broths of chicken cholera, freed from all micro-organisms by heat, confer immunity on fowls; these researches date from the years 1885 and 1886.

In the month of October, 1887, Charrin, in communications made to the Academy of Sciences, demonstrated the same fact with reference to the *pyocyanic bacillus*, and when you inject under the skin of the hare a filtered culture of this microbe, you confer on the animal immunity against intravenous injections of the infectious liquid.

It was still in the same year, 1887, that Peyraud,† of Libourne, communicated to the Society of Biology his curious experiments on tansy and the convulsive phenomena which it determines in animals. He endeavors to show that these phenomena resemble those of canine madness. He gives to this kind of poisoning the name of *tansy madness*, and maintains that by injecting this substance under the skin, you may confer on animals immunity from street rabies. Peyraud also labors to find the leucomaïne of rabies, and compares the rabic poison to the essence of tansy.

Lastly, Roux and Chamberland published in November, 1887, their important researches on gangrenous septicæmia, and showed that culture-broths of the septic vibrio injected in animals confer immunity from septicæmia. Chantemasse and Widal also made known in February, 1888, that it was possible by the leucomaïnes secreted by the bacillus typhosus to confer immunity against typhoid infection. Lastly, Gamaleia, of Odessa, affirmed, in January, 1888, the same fact in regard to cholera. Charbon itself, according to the experiments of Roger, belongs to the group of infectious diseases from which immunity may be attained by the employment of a chemical vaccine obtained from the culture liquids.

As you see, this question of chemical vaccines tends to become more and more important every day.

How are we to explain this curious action of the leucomaïnes secreted by the microbes in conferring immunity on animals?

Must we, as Bouchard believes, separate the vaccinal from the virulent properties? Must we, on the other hand, assume that the same chemical substance is at once vaccinal and virulent, and if it renders the organism refractory to subsequent inoculations, it is because the animal is, as has been said, *mithridatized*,‡ that is to say, because the economy has become habituated to and proof

* "Therapeutique des Maladies Infectieuses." Paris, 1889.

† Peyraud, "On Immunity by Chemical Vaccines." Paris, 1888.

‡ An acquired tolerance of poison.—[TR.]

against the toxines secreted by the pathogenic microbe?

These are questions of great importance which are far from being solved, and the discovery of chemical vaccines is of too recent date to warrant us in expecting any very immediate explanation, despite the ardor of the researches undertaken in this direction. However, this may be, we must admit, as demonstrated to-day, the reality of these chemical vaccines, and it is probable that this subject will soon pass from the domain of the laboratory to that of practice.

Let us examine now what practical results have been furnished by the Pasteurian vaccinations; we will consider them chiefly in connection with two diseases, anthrax in domestic animals, and rabies in man.

As for anthrax, we have demonstrative figures furnished by Chamberland to the Congress of Vienna. In reply to the opponents of the anthracoid inoculations who objected that these inoculations give no practical results, Chamberland furnished the following statistics:

RESULTS OF ANTHRACOID VACCINATIONS.

SHEEP.

	Animals vaccinated.	Mortality during vaccination. Per cent.	Mortality after vaccination. Per cent.
1882.....	243,199	.65	.45
1883.....	193,119	.36	.41
1884.....	231,693	.47	.50
1885.....	280,107	.57	.33
1886.....	202,064	.46	.29
Average...	.....	.50	.40

HORNED CATTLE.

	Animals vaccinated.	Mortality during vaccination. Per cent.	Mortality after vaccination. Per cent.
1882.....	21,916	.15	.....
1883.....	20,501	.09	.20
1884.....	22,616	.15	.22
1885.....	30,428	.19	.14
1886.....	22,376	.11	.17
Average...	.....	.14	.19

These figures are eloquent, and show that in their application to practice, the Pasteurian vaccinations give remarkable results, and that they are destined to become more and more general.

But the question which most interests us is, that which pertains to canine madness and hydrophobia; this is the first practical application to man of the Pasteurian vaccination.

Let us see, first, what was the mortality of rabies before Pasteur's method came into use. The figures are very variable. Thus, according to Gowers, the mortality was forty to fifty per cent. of persons bitten; according to Renault, thirty-three per cent.; Bouley places the figure at forty-seven per cent.; lastly, Leblanc gives it as sixteen per cent. If you take as correct the lowest of these estimates, and this is probably nearest the truth, you will be obliged to admit that the results obtained by Pasteur's method are most gratifying.

Having been for several years commissioned by the Council of Hygiene and Salubrity of the Department of the Seine to examine into the cases of hydrophobia there occurring, I have in successive reports communicated the facts given below. Thanks, on the one hand, to the accurate lists kept by the commissaries of the police, who make a record of all persons bitten by mad dogs, and, on the other, to the registers of the Pasteur Institute, it has been an easy matter to obtain absolutely reliable statistics of the past two years.

In 1887, three hundred and six persons presented themselves at the Pasteur Institute; they are classed as follows:

Sixty-four had been bitten by animals which had been experimentally proved to be victims of rabies; one hundred and ninety-nine by animals whose madness had been attested by veterinary surgeons; forty-three by animals concerning which we could get no definite information.

Of these three hundred and six persons there have died: two in 1887; one in 1888. This gives a mortality of .97 per cent., or 1.14 per cent. if you take into account only the two hundred and sixty-three persons bitten by animals known certainly to have been mad.

During the same year, there were forty-four persons bitten by rabid animals who were not brought to the Pasteur Institute; out of this number there were seven deaths, giving a mortality of 15.90 per cent.

The statistics for 1888 are as follows:

Three hundred and eighty-five persons presented themselves at the Pasteur Institute. Of these, one hundred and five had been bitten by animals whose madness had been experimentally demonstrated; two hundred and thirty-one by animals which had been pronounced victims of rabies by veterinary surgeons; forty-nine by animals concerning which we had no definite information.

These three hundred and eighty-five persons have furnished a mortality of five,—four in 1888 and one in 1889; this gives a percentage of 1.29, and, if you take into account

only the three hundred and thirty-six persons bitten by dogs known to be mad, you have a mortality of 1.49 per cent.

During the same year, there were one hundred and five persons bitten by mad dogs who did not present themselves at the Pasteur Institute; of these, fourteen died, giving a mortality of 13.3 per cent.

To sum up, the mortality in persons not treated by Pasteur's method was 15.90 per cent. in 1887, and 13.36 per cent. in 1888. These figures, as you see, come very near to those given by Leblanc. Among those treated by preventive inoculations who had been bitten by dogs concerning whose madness there was no doubt, the mortality was 1.14 per cent. in 1887 and 1.49 per cent. in 1888. It is then plain that the benefits of the Pasteurian treatment are indisputable, since the mortality is brought down from an average of fifteen or sixteen per cent. to about one per cent.

These results have not pertained exclusively to the Institute Pasteur. Recall to mind, in this connection, the discourse pronounced by Grancher at the inauguration of the Institute Pasteur. You will there find statistics which agree absolutely with those which I have just given you, and you will see that the mortality for the totality of individuals who presented themselves at the Pasteur Institute in 1886 was 1.34 per cent., and in 1887 1.12 per cent. Moreover, Pasteurian institutes are being established in other countries, and we may reckon as many as twenty in the entire world. At Odessa, under the direction of Gamaleia, the mortality was 1.11 per cent. in 1886, 1887, and 1888; at Moscow, it was 1.27 in 1887 and 1.60 in 1888; at Naples, it has been 1.5, and in Havana, 1.6.

We may, then, conclude from past experience that wherever the Pasteurian method has been carried out, the mortality has fallen, on an average, to about 1.5 per cent. This is an unquestionable gain, and we cannot too much emphasize these figures when in the presence of those who gainsay Pasteur's method.

When, instead of applying the antirabic inoculations to man, animals are made the subjects of these inoculations, the results are still more conclusive. In the experiments made by Pasteur we find facts like the following: He takes fifty dogs, on which he performs the preventive inoculations, fifty other dogs serving as tests. When the Pasteurian treatment is finished, he inoculates these hundred animals with street rabies by intracranial inoculation. The fifty that were not inoculated all succumb; not one of the others becomes rabid.

This is not all. One may even arrest, in animals as in man, the commencing development of rabies experimentally induced by employing the preventive inoculations. But here, as in the case of man, there is always a certain mortality.

I might pursue this subject considerably further, but I think that I have sufficiently shown you the benefits which medicine is likely to derive from the vaccination methods discovered by our illustrious fellow-countryman. The era of vaccinations has but just begun, and everything warrants the expectation that in proportion as these preventive methods are perfected, they will constitute one of the principal elements for the preservation of man and of animals from infectious diseases.

BICARBONATE OF SODIUM AND BICHLORIDE OF MERCURY IN THE TREATMENT OF YELLOW FEVER.

By Geo. M. Sternberg, Major and Surgeon, U.S.A.

In a "preliminary note," published in The Therapeutic Gazette of August 15, 1888, the writer gave the results of a mode of treatment suggested by himself while in Havana last year. The cases referred to in this note were treated at the Garcini Hospital by Dr. Raphael Weiss, to whom I am indebted for clinical histories of twelve successive cases treated without a single fatal result.

The object of the present paper is to record the results of a more extended trial of the same treatment during the recent epidemics at Decatur, Alabama, and at Jacksonville, Florida.

Upon my arrival in Decatur, in the early part of October last, I found that yellow fever of a most malignant type was prevailing, and that the mortality had been very great even under the most favorable conditions as to nursing and surroundings. This is shown by the fact that out of ten physicians practising in the infected area, nine had yellow fever and five died. The one who escaped, Dr. B. F. Cross, had suffered an attack during a previous epidemic in the same place (1878). Soon after my arrival I called attention to the alkaline and bichloride treatment, and, in view of the very unsatisfactory results which had been obtained by other methods, the physicians remaining on duty, two of

whom were recent convalescents, one after another adopted the treatment. Since the termination of the epidemic they have kindly given me a statement of the results obtained and more or less complete clinical histories of their cases.

The formula originally prescribed by me, as published in my preliminary note in the THERAPEUTIC GAZETTE, was adhered to. This is as follows:

℞ Sodii bicarb., grammes x (grs. 150);
Hydrarg. chloridi corrosiv., centigrammes ii ($\frac{3}{10}$ gr.);
Aquæ puræ, litre 1 (1 quart). M.
Sig.—50 grammes (about 1¾ oz.) every hour; to be given *ice cold*.

RESULTS OF TREATMENT IN DECATUR.

Treated by	Whites.			Colored.		
	M.	F.	Died.	M.	F.	Died.
Dr. B. F. Cross.....	13	7	2	2	2	0
Dr. E. J. Conyngton.....	5	3	1	1	2	0
Dr. E. M. Littlejohn.....	3	...	0	9	7	0
Dr. W. C. Buckley.....	1	...	1	5	4	0
Total.....	22	10	4	17	15	0

I have excluded from the above tabular statement two cases treated by Dr. Cross, in which the treatment was not commenced at the outset of the attack. One of these, a white male, terminated favorably; the other, a white female, resulted fatally. Dr. Cross remarks, with reference to the last-mentioned case: "She was taken sick on the 29th of September and died November 10. Was not put on the Sternberg treatment until the latter part of her illness."

The two fatal cases in the practice of Dr. Cross were: No. 1, Mr. J., aged about 30, taken sick October 18. Died October 23 after suppression of urine and black vomit. No. 2, Mrs. D., a feeble woman of 58 or 60, who had recently lost her husband from the same disease.

The fatal case in Dr. Conyngton's practice was: Captain K., "aged about 50 years; a large, plethoric man, rather a heavy drinker; had been in the habit of drinking whiskey for thirty years." This patient had suppression of urine on the fourth day of sickness.

The fatal case treated by Dr. Buckley was: Mr. B., aged 33. "The patient had a severe case of yellow fever, but was doing well, considering the severity of his case, until the morning of the seventh day, when he was found much worse. . . . It was ascertained that the family gave him a hearty meal on the sixth day,—the one previous to his becoming worse."

Through the kindness of Dr. Jerome Cochran, State Health Officer, I have received the following official statement of the number of cases and deaths in the Decatur epidemic:

Whites: { Cases, 97. Deaths, 30. } Colored: { Cases, 57. Deaths, 5. }

This gives us a general mortality of 29.22 per cent., and a mortality among the whites alone of 30.92 per cent.

Reference to the above table will show that in a total of sixty-four cases treated by the alkaline and bichloride method the mortality was only 6.45 per cent., and that the mortality among the whites, considered separately, was 12.5 per cent.

A just comparison, however, requires that we shall deduct the number treated by the method under investigation from the total number of cases occurring during the epidemic. This leaves us sixty-five whites with twenty-six deaths, and twenty-five colored with five deaths, treated by other methods; and gives a mortality of forty per cent. among the white and twenty per cent. among the colored population; whereas, under the alkaline and bichloride treatment not a single death occurred out of thirty-two cases among the colored population. Many of the cases were bright mulattoes—and the mortality among the whites was reduced to 12.5 per cent.

RESULTS OF TREATMENT IN JACKSONVILLE, FLORIDA.

My "preliminary note" in the THERAPEUTIC GAZETTE was brought to the attention of some of the physicians in Jacksonville during the recent epidemic, and I am informed that a number of physicians employed the treatment referred to, and that it gave general satisfaction to those who adopted it. I have as yet only obtained a detailed report from two physicians; but as these reports include a considerable number of cases treated in the Sand Hills Hospital by Dr. Sollace Mitchell, and in private practice by Dr. A. J. Wakefield, they may be taken as fairly representing the general results obtained by this method of treatment during the Jacksonville epidemic.

Dr. Mitchell writes me as follows:

"JACKSONVILLE, FLA., September 9, 1889.

"G. M. STERNBERG, M.D., BALTIMORE, MD.:

"MY DEAR DOCTOR,—I send you to-day a list of all cases treated upon the bichloride

and alkaline treatment; also those upon the triturated bichloride without the alkali. I confess my surprise at the great difference in mortality. I knew that my results with the alkali were far better than without it, but did not realize the great difference until I made out these lists for you. Some of the ages have been approximated because the book containing the ages, reception, etc., of many has been lost or mislaid. I treated in all two hundred and sixteen cases. I did not begin the use of the bichloride until I had treated some thirty-five or forty by other methods, and when the bichloride was begun only every fourth patient was put upon it, then every other patient, and towards the last almost all patients were put upon it [*i.e.*, the alkaline and bichloride treatment.—G. M. S.]. Cases were chosen by rotation as they came in, different treatment being used in different wards. The bichloride and alkali gave the best results by all odds.

"Most sincerely yours,

"SOLLACE MITCHELL."

The lists transmitted by Dr. Mitchell give the name, age, color, sex, and result, with a remark as to the severity of each case.

The list of those treated by the alkaline and bichloride method includes one hundred and six cases with five deaths,—a mortality of 4.7 per cent.

Seventy-nine of the cases and all of the deaths were whites,—a mortality of 6.3 per cent. Twenty-seven cases were colored, with no deaths.

Of the whites, seventy-three were males and six females. The deaths all occurred among the white males, and the mortality among this class, considered separately, was 6.8 per cent. This is certainly a very unusual result, and brings out in a very striking manner the value of this mode of treatment. Yellow fever is well known to be especially fatal among adult males, and in hospital practice a mortality of less than twenty-five per cent. among this class of cases is exceptional.

The mortality lists published in the newspapers during the Jacksonville epidemic gave the impression that the epidemic was of an unusually mild character. But it must be remembered that these lists related to the population generally, and included a very large proportion of the colored race, who remained in the infected city, while a large share of the white population left it upon the outbreak of the epidemic. Now, the mortality from this disease among negroes is always small, and is estimated by Dr. Mitchell not to have been over two per cent. in the Jacksonville epidemic. Yet the general mortality, as shown by the daily reports published in the newspapers, was in the vicinity of ten per cent. Dr. Mitchell estimates the mortality among the white population, considered separately, as from twenty-two to twenty-five per cent., but has not yet been able to give me the exact figures. This estimate includes both sexes and all ages, and if adult males were considered separately it would no doubt be considerably more than this. With two or three exceptions, the seventy-nine whites included in Dr. Mitchell's list were adults; one only was a child of 8 years; ten were between 15 and 20; twenty-five, from 20 to 30; twenty-nine, from 30 to 40; eleven, from 40 to 50; and three, from 50 to 60.

The cases are classified by Dr. Mitchell, as to severity, as follows: "Mild," forty-six; "severe," nineteen; "very severe," fourteen. It must be remembered that the treatment was commenced at the outset, and the mildness of the attack in those cases classified as "mild" may have been due to some extent to the favorable influence of the medicine administered. Indeed, the general result indicates that such was the fact. It is well known to those physicians who have had an extended experience in the treatment of yellow fever that many cases which appear to be mild at the outset in the end terminate fatally, and especially so in adult males, among whom the tendency to suppression of urine seems greater than in the case of females and children.

One of the principal objects which I had in view in prescribing full doses of bicarbonate of sodium, at regular intervals, was to neutralize the very acid condition of the urine, in the hope that it might be secreted more abundantly and with less injury to the renal parenchyma. The results attained seem to indicate that such is the case, and that the favorable action of the medicine is largely due to this fact.

Dr. Weiss, in transmitting the clinical notes of twelve successive cases treated in the Garcini Hospital, in Havana (in 1888), without a death, says:

"The urine has always been abundant. In some cases the density was lowered, but was compensated by a large quantity of urine. Albumen did not appear in some of the cases, in others the amount was small, and in others it reached two grammes to the litre. The albumen has always disappeared on the second or third day, and then the fever ceased."

Dr. Conyngton, of Decatur, remarks:

"One point of special note was that patients on this treatment in my hands never had any symptoms of suppression of urine, but rather the quantity was increased; while, on the other hand, in patients treated by the expectant plan, with diuretics, the urine was scanty, and those that died (three in number) died of suppression."

Dr. Sollace Mitchell, in a letter dated February 1, speaks of the "marked effect of the treatment" in diminishing the albumen and preventing in a great measure suppression of urine.

Dr. A. J. Wakefield, of Jacksonville, has given me a list of eighty-nine cases treated by himself during the recent epidemic in that city. Of these cases seventy-five were white and fourteen colored. Five deaths occurred among the whites,—a mortality of 6.6 per cent. Thirty-nine of the white cases were males and thirty-six females. Forty-one of the cases are classed as severe and forty-eight as mild. One of the fatal cases is said to have been a consumptive; one to have been convalescent and to have died from imprudence; in another the remark is made, "bad nursing and imprudence;" another, "unfavorable surroundings."

RESULTS OF TREATMENT AT RIO DE JANEIRO.

My friend, Dr. Robert Cleary, an American physician practising in Rio de Janeiro, in a letter dated December 13, 1888, gives me an account of four cases treated by this method; three made a good recovery and one died. The fatal case assumed a severe form from the outset, and was attended with hemorrhage from the vagina. Black vomit and suppression of urine occurred on the fourth day. Dr. Cleary thinks that in this case no treatment would have been of any avail, and says, in conclusion, "Should any new cases present themselves to me I shall continue using your method, as I believe in it."

I have recently received a clipping from the *Journal of Commerce* of Rio de Janeiro, containing a note from Dr. Rocha Faria, Inspector of Hygiene, in which the formula prescribed by me is given and strongly recommended. Dr. Faria says, "Some physicians who, during the present epidemic, have followed this treatment have obtained magnificent results."

The question arises as to whether the favorable results which have thus far attended this mode of treatment are due to the alkaline element of the treatment, or to the antiseptic element, or to the combined action of the two ingredients.

My principal object in suggesting the formula was to test a decidedly alkaline treatment from the outset of the attack, with a view to relieving the gastric distress and acid vomiting which is a prominent feature in cases treated by the expectant method; and also to render the highly-acid urine neutral or slightly alkaline, in the hope that the secretion would be more abundant and the tendency to suppression diminished. The treatment appears to meet these important indications; and, without in any sense being "specific," to save life by preventing those structural changes which give rise to passive hemorrhage from the stomach and suppression of urine,—two symptoms which present themselves in a majority of the fatal cases.

Bichloride of mercury in a comparatively small amount was added to the formula, not with the idea that it would to any extent destroy pathogenic micro-organisms in the intestine, but as an antiseptic which might be useful in preventing fermentative changes in the stomach, which would perhaps be favored by the free administration of an alkali. The idea has also occurred to me that the specific germ may possibly find a suitable nidus in the acid secretions of the stomach, and in this case the administration of an antiseptic in combination with an alkali would be the most rational treatment. Still, I have not given much weight to this idea. In my "preliminary note" I say, "I am rather disposed to attribute the favorable course of the disease to the full alkaline treatment than to any antiseptic effect of the bichloride in the intestine. In the stomach, however, I should expect a decided antiseptic effect from the medicine as administered, and perhaps this is, after all, what is most needed." I insist upon this point because the treatment has frequently been referred to by those who have used it as the "bichloride treatment." From my point of view it is essentially *an alkaline treatment*, with the addition of a certain amount of the bichloride of mercury as an adjuvant to meet certain indications, but which is not administered with the expectation that, by virtue of its germicide power, it may prove a specific in this disease.

Bichloride of mercury has, however, during the past year been administered in considerably larger doses than I have prescribed in the above formula, with the express purpose of destroying pathogenic micro-organisms in the intestine. This treatment was proposed

by Dr. Paul Gibier during his stay in Havana last year, and has been fairly tested by Dr. Vincent de la Guardia, in his wards in the charity hospital of this city.

The idea that the specific infectious agent of yellow fever may, as in cholera, have its habitat in the alimentary canal, has been forced upon the writer by his failure to find it in the blood and tissues of those suffering from the disease. Dr. Gibier has been led to a similar inference, and has even claimed the discovery of a bacillus which, as we think upon insufficient evidence, he supposes to be the specific infectious agent in question. The treatment proposed by him is based upon this idea, and consists in the free administration of purgatives for the purpose of clearing the pathogenic microbes out of the intestine, and of bichloride of mercury in large doses for its germicide effect. So far as the purgation is concerned, experienced physicians in the yellow fever zone generally agree that it is important to clear out the bowels with a brisk purge at the outset of the attack; but it is also established by experience that active medication of this kind after the first or second day of an attack is likely to do more harm than good, and, in fact, is a dangerous procedure.

I quote from the paper of Dr. de la Guardia, published in the *Crónica Médico-Quirurgica*, of Havana, as follows:

"We have been constant in giving the patients the series of purges recommended; the first day a saline cathartic,—45 grammes of sulphate of sodium; the second day, 45 grammes of castor oil in emulsion, with an equal quantity of syrup; and, finally, on the third day, 1 gramme of calomel, to which we added 1 gramme of jalap to make it more active,—given in two doses at an interval of one hour. The saline purge was frequently rejected by vomiting, and this also happened sometimes with the castor oil,—not so with the calomel, which is generally tolerated. During the period of large evacuations we invariably made use of the mercurial medication; using a draught composed of 5 centigrammes of bichloride of mercury, 30 grammes of brandy, and 120 grammes of sweetened water. Some patients rejected this by vomiting immediately after taking it. The patients were made to take this until the seventh, or, in some cases, the eighth day; at this time the favorable or unfavorable result was decided. During his illness the patient was given a large quantity of liquid in the form of chlorhydric lemonade (2 to 1000) iced. This was well retained by the stomach. Milk was the only food allowed,—generally iced. In some of the most severe cases the purgatives were again commenced. Certain symptoms were treated by special measures, in accordance with general principles. Vomiting, for example, was treated by the application of a blister to the epigastrium, and the patient was allowed to swallow small pieces of ice, Rivier's draught, etc. Hemorrhage was treated with ergot, and adynamia by tonics. The bichloride of mercury has rarely given rise to stomatitis, and when this has happened it has been mild in character and has been rapidly cured with simple emollient washes, or with chlorate of potassium.

"Thirty-seven cases were submitted to the mercurial treatment, all of them typical cases of yellow fever, in Saint Margaret's Ward of the Civil Hospital. Of these twenty-two recovered and fifteen died. This gives a mortality of forty per cent., which is about the average mortality from this disease in Havana,—a little more or less, but rather more.

"It will be seen that the result of our scrupulous investigation up to the present time has been, if not disadvantageous, at least without any advantage to the patients treated."

In a letter from Dr. Sollace Mitchell, from which I have already quoted, he says, "Dr. Gibier claims to have suggested the use of bichloride internally some weeks prior to yourself; he says that he suggested $\frac{1}{10}$ grain every hour; but as I experimented with doses from $\frac{1}{20}$ to $\frac{1}{60}$, and found that in six cases when $\frac{1}{20}$ was used severe cramps and diarrhœa followed in from six to eighteen hours, I doubt very much if a patient could bear $\frac{1}{10}$ grain with advantage to himself."

Dr. Mitchell has given me a list of forty cases treated by the bichloride alone; thirty-two of these were whites and eight colored; nine of the whites died,—a mortality of twenty-eight per cent; whereas the mortality among the same class of patients treated by the alkaline and bichloride method was only 6.3 per cent.

Dr. Mitchell has given the following summary statement of the results obtained by the two methods of treatment:

"One hundred and six cases, bichloride and alkaline treatment, with five deaths, and one recovery from black vomit. Forty cases, bichloride treatment, with nine deaths, and twenty-five recoveries from black vomit."

Now, as Dr. Mitchell has stated in his letter that the cases were not selected, but taken

"alternately" to test the various methods of treatment, it is evident either that in the first series of one hundred and six cases, with one recovery from black vomit, the absence of this symptom was due to the treatment employed; or that in the second series of forty cases, with twenty-five recoveries from black vomit, this symptom was, to some extent at least, a result of the mode of treatment. The latter supposition seems the most probable one, because the number of cases of black vomit reported is excessive, and if these cases had been treated by the expectant method, experience in other epidemics indicates that there would not have been so large a number of non-fatal cases characterized by this symptom. I infer, then, that the bichloride treatment, as tested in this series of cases, is liable to induce passive hemorrhage from the gastric mucous membrane. And, on the other hand, the first series of cases gives support to the belief that the tendency to such passive hemorrhage, which is a prominent feature in this disease, is diminished by the administration of full doses of bicarbonate of sodium from the outset of the attack.

I have but a single series of cases to report in which the bicarbonate of sodium was administered alone. These cases were treated at the "Quinta La Purissima Concepcion" by Dr. Duran. The formula used was: Bicarbonate of sodium, 6 grammes; sweetened water, 250 grammes. Of this mixture two tablespoonfuls were administered every two hours, or about 9 grammes (135 grs.) of the bicarbonate of sodium in the twenty-four hours. I have no information as to the character of the cases, or as to how long the treatment was continued. It often happens in the private hospitals of Havana that the patients, when brought to the hospital, have already been sick for several days, and the mortality is greatly influenced by this fact. In all, thirty cases are reported as having been treated by this solution, with eight deaths,—a mortality of 26.6 per cent. At the same time eleven cases were treated by the "antiseptic method," with six deaths,—a mortality of 54.5 per cent. The antiseptic administered was biniodide of mercury, 2 centigrammes, in 250 centigrammes of sweetened water,—two tablespoonfuls every two hours.

It will be noted that the amount of bicarbonate of sodium in the formula used at the Quinta La Purissima Concepcion was less than in my original prescription. I am disposed to think that it would have been better to increase the dose. This was done by Dr. Sollace Mitchell in the Sand Hills Hospital, at Jacksonville, and his results fully justify his judgment in this regard. In making my formula at first, in the absence of any clinical experience in this disease with the special remedies I proposed to use, I felt that it would be wise to keep within the bounds of perfect safety, but Dr. Mitchell has shown that the doses of bicarbonate of sodium may be considerably increased, not only with safety, but with excellent results. He says, "The formula that I finally adopted was:

℞ Sodii bicarb., grs. x to lx;
Hyd. chlor. corros., gr. $\frac{1}{10}$;
Aquæ puræ., ℥iv. M.

Give, *ice cold*, every hour during the day and every two hours at night."

This, taking the minimum amount,—10 grains,—would make three drachms of bicarbonate of sodium and $\frac{6}{10}$ of a grain of bichloride of mercury in the twenty-four hours. I prefer to prescribe a sufficient quantity for the twenty-four hours and to give the medicines more largely diluted. I noticed at Decatur that the patients not only took the mixture as originally prescribed by me without objection, but that they looked forward to the time when the medicine was to be administered, as they found the ice-cold drink refreshing. And as vomiting has not, in my observation, been induced, but, on the other hand, gastric irritability has been allayed, I can see no objection to giving the hourly dose in 1½ to 2 fluidounces of water. Indeed, I think that the taking of a considerable amount of fluid, if it is promptly absorbed by the stomach, is rather beneficial than otherwise. My original prescription contained an amount of water—1 litre—which in my judgment the patient might be permitted to drink, in divided doses, in the course of the twenty-four hours. Under certain circumstances, of course, this might be increased by giving small draughts of cold water, or bits of ice, in the intervals between the administration of the medicine. But excess in this direction will often induce vomiting, and I have desired to avail myself of the advantage of a formula which contains a proper amount of water, to be administered at short intervals, instead of leaving the administration of this fluid to the judgment of the nurse or the desires of the patient. In this way, also, the whole treatment becomes extremely simple and is easily carried out,—no small advantage for the busy practitioner during the prevalence of an epidemic.

In view of these considerations and of the experience of Dr. Sollace Mitchell with increased doses of the two ingredients contained in my original formula, I would suggest for further trial the following:

℞ Sodii bicarb., grammes xvi (℥iv);
Hyd. chlor. corros., centigrammes iii (gr. ½),
Aquæ puræ, litre 1 (1 quart). M.
Sig.—50 grammes (about 1¾ oz.) every hour; to be given *ice cold*.

Naturally the amount given should vary according to the age of the patient, and perhaps, also, with reference to the severity of the case. Thus a mild case might have two tablespoonfuls every hour during the day and every two hours at night; while a case which at the outset appeared to be of a grave character might take two fluidounces of the mixture every hour, day and night. Experience may show that the dose of bicarbonate of sodium may be still further increased, in severe cases, with benefit to the patient; but I doubt, in view of the evidence above recorded, whether there would be any advantage in increasing the dose of mercuric chloride. It would be a mistake to attempt to substitute a potassium salt for the bicarbonate of sodium in the above formula. The mercuric chloride, which remains in solution in presence of sodium bicarbonate, in the proportions prescribed, would be precipitated by carbonate of potassium. Moreover, the potassium salts are directly contraindicated in a disease in which there is a great tendency to suppression of urine and uræmic poisoning. The experiments of Zelz and Ritter, and of Bouchard, show that in uræmia the toxic symptoms are largely due to the retention of potassium salts rather than to urea. Bouchard, especially, as a result of his extended experiments, insists upon "the great importance of limiting potassium salts both in food and medicine" in the treatment of uræmia.

I may add that a rather extended trial of carbonate of potassium in full doses, made by one of the physicians of Decatur during the recent epidemic, was attended with a high rate of mortality, and that the physician referred to himself fell a victim to the disease, although his case at the outset appeared to be rather a mild one. How far the treatment influenced the result it is, of course, impossible to determine, but suppression of urine, and black vomit occurred on the fourth day, and death quickly followed.

HAVANA, April 6, 1889.

*REMARKS ON SPINAL INJURIES, MORE ESPECIALLY "RAILWAY SPINE," WITH HINTS ON EXPERT TESTIMONY.**

BY F. X. DERCUM, M.D., PHILADELPHIA, PA.†

GENTLEMEN:—Numerous interesting topics, especially the various medico-legal relations of insanity, have been discussed *in extenso* by this Society, but, up to the present time, the extremely important subject of railway injuries has received little, if any, attention. You are all, doubtless, aware that annually many thousands of dollars are paid by railway companies, in the form of damages, for injuries claimed to have been received. The sums that have been paid out are in the aggregate very large, it being estimated by one writer that in the course of about a dozen years at least twenty million dollars have been paid out in this country and in England. Large as this amount seems, it should in nowise prejudice us in approaching the subject either as a whole or in testing the merits of any individual case. Let us bear in mind that whether this sum is to be looked upon as indicative, on the one hand, of the excessive number and frightful character of railroad accidents, or, on the other, of fraud on the part of plaintiffs, or of excessive sympathy on the part of juries, it can in no way enter into the discussion before us. At most, it merely illustrates the magnitude of the subject, or, perhaps, impresses us with the very great importance of expert testimony. How important this testimony is, few will fail to realize. The result in many, if not in most, cases, hinges entirely upon it. The accuracy of statement, the nicety of judgment, the personal integrity of the expert not infrequently turn the scale.

It is with some diffidence that I bring this subject before you. It is so extensive that I can only deal with its various parts briefly and in a general way. A volume would be required to do it justice. Further, the position I hold to-night is one of peculiar difficulty. In order to meet the needs of the occasion, I must speak as much as possible in language which our legal brethren, who are not supposed to have a knowledge of medical terms, can understand, while at the same time I must be technically accurate. Much also

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of what I shall say would be omitted in an address before a purely medical society, but which, if omitted here, would make my discourse unintelligible to one-half my hearers.

The injuries to which I invite your attention are injuries of the spine, more especially those which come under the head of "railway spine." The term railway spine is in many respects objectionable. It may convey the erroneous impression that there is some one clearly-defined affection of the spine to which it should be restricted, while in truth the injuries of the spine liable to occur in railway accidents are most various. Further, the expression implies that there is something peculiar in the injuries produced by railways, and that they differ from injuries from other causes, whereas the most varied accidents may produce identical conditions. Such, for instance, may be the case with a simple fall upon the back, the shock of an explosion, or a blow received from the timbers of a falling building. However, the term railway spine is in common use, and as long as we interpret it as signifying in a general way the various conditions resulting from blows, falls, wrenching, or jarring of the back, we run no risk of creating confusion by its use.

Injuries of the back are, of course, various in kind and degree. We may have in one case merely a bruise of the back, in another an actual fracture of the spinal column, and in another, still, marked nervous symptoms, without any visible signs of injury whatever. We find, then, at the very outset of our inquiry, a crude yet practical classification of spinal injuries presenting itself. In the first group we have those cases in which the various tissues of the back, the ligaments, nerves, and muscles, have been bruised or have otherwise suffered from strain,—*i.e.*, cases in which there has been no actual damage to the spine or its contents, but merely to the tissues immediately surrounding or in proximity to it. In the second group, in addition to fracture of the spine, we should include dislocation of the vertebræ at the time of the accident. It should be remembered that in these cases hemorrhage into the spinal marrow or its coverings and spinal concussion may occur as complications. The third group will include all those cases in which nervous symptoms are marked, but in which external proof of accident may be wanting,—*i.e.*, it includes, first, cases of injury to the spinal contents, such as concussion, hemorrhage, and late oncoming inflammation and degeneration; and, secondly, those cases in which no other symptoms are present than those of a persistent loss of nervous tone or that unstable state which we call hysteria.

You will say at once that groups two and three overlap; and, again, as regards groups one and two, that a blow sufficient to break the back must of course be sufficient to bruise the muscles and ligaments: in other words, that the various groups I have made merge into one another. Classifications are merely pegs on which to hang our ideas, and I believe that such a one as I have given you is more useful for our present purposes than a more elaborate classification based solely upon symptomatology.

Regarding group one, I shall have very little to say, as the diagnosis of the condition is relatively easy. The skin of the back is very dense and thick, and frequently powerful blows produce little discoloration, so that it not infrequently happens that in cases where the underlying muscles and ligaments have severely suffered, the skin itself exhibits no change. The muscles, on the other hand, occasionally exhibit decided swelling and distortion. The principle symptoms, however, on which we have to rely are excessive tenderness and pain on motion. Sprains of the ligaments are indicated by great rigidity of the spine, intense pain on motion, and often by involuntary spasm of the various spinal muscles, the spasm being likewise provoked by attempted motions. In some cases transmitted shock—*e.g.*, a slight vertical blow upon the head—causes acute suffering at the point of injury.

As you would naturally expect, these sprains of the back are more apt to be caused by accidents, which cause sudden flexion or sudden twisting of the body, and the history of the accident should, therefore, be taken into account in investigating a doubtful case.

As regards the outcome of these cases, it may be said that very many of them get well, but that rest in the recumbent posture for weeks, and often months, is necessary. Sprains of the muscles present, of course, a much more favorable outlook than sprains of the ligaments. The following cases, taken from my own experience, will illustrate this point: A medium-weight woman, aged 55 years, fell down a short flight of stairs, landing in such a way that the lumbar region struck the last step with great force. In addition to the temporary shock and nervousness of the accident, the patient complained of great pain in the region of the left quadrate lumbar mus-

cle. Inspection revealed it to be much swollen and exquisitely tender, and in its lower third a distinct groove or ditch could be made out by the fingers, as though some of the fibres had parted from their tendinous insertion into the innominate bone. The patient was kept in bed over four weeks, and even at that time tenderness and pain had not altogether disappeared. Another case is that of an old gentleman, 78 years of age, but in excellent health and of a very long-lived stock. He was thrown forward, while standing, by the unexpected starting of a train. He fell at full length, striking in his course his forehead against the door of the car in such a way that his head and neck were bent violently backward. When I saw him his head was held in an absolutely fixed position, while the tissues of the neck were everywhere exquisitely sensitive to deep pressure, and the slightest jar would give excruciating pain. The muscles of the back of the neck were hard and rigid, as though in constant spasm. In this case, the man being in comfortable circumstances, the question of a suit for damages was never raised, and he certainly was very anxious to get well. Several months afterwards, when last heard from, there had been no change in his condition. It is extremely probable that in this case the dense fibrous tissues comprising the ligaments of the spinal column, the intervertebral cartilages, and the numerous tendinous insertions of the muscles had been strained, so that the outlook, as far as speedy recovery was concerned, was not very favorable. It is worthy of remark that at no time were there any symptoms referable to injury of the spinal marrow.

I might multiply these cases almost indefinitely by citing from the records of various writers, especially the cases of Hilton.* However, my object to-night is to give expression to general principles rather than to detail.

In sprain of the back or spine, therefore, the symptoms to be sought for are rigidity, pain, tenderness, and frequently involuntary spasm of muscles on attempting passive movement. Tenderness should be sought for over the muscles and with deep pressure. The expert must be careful in doubtful cases not to confound this condition with hysterical hyperæsthesia affecting the skin, or with the so-called painful spine of hysterical and neurasthenic subjects; the tenderness in these instances is, of course, superficial.

* Hilton, "On Rest and Pain." Wood & Co., 1876.

In giving a prognosis in sprained back, the expert should be guided, in addition to general principles, by the fact that cases of mere muscle sprain progress more favorably than those in which the ligaments have suffered, and especially, do I think, should he be guarded in his opinion if pain is readily excited by transmitted shock; the latter symptom suggesting among other things injury of the intervertebral cartilages or the bodies of the vertebræ themselves. Further, nervous symptoms, such as I shall speak of later, should be carefully sought for, in order to exclude possible injury of the spinal contents. Again, the relation of blows and falls to the development of Pott's disease, is well known, and the expert should bear it in mind. An interesting illustration of this often late oncoming affection is afforded by a patient now in the wards of the Philadelphia Hospital. He is a middle-aged man, who, while walking, was struck on the shoulders by a heavy bale of rags, the bale falling from the height of a third floor. The blow caused him to sit down suddenly on the pavement, while his thorax was forcibly flexed upon his abdomen. After recovering from a momentary shock, he walked to his home. Here he remained a little over two weeks, suffering the while with pain in the back. At the end of this time he resumed his occupation,—that of a carpenter,—and the seriousness of his injury was not made evident until seven months after, when he began to lose power in his legs. At present he is decidedly paraplegic, and he suffers from marked displacement of the last dorsal and first lumbar vertebræ.

Another point to be remembered is, that severe contusions of the back are sometimes followed by bloody urine, due presumably to laceration of the tissue of the kidney. This symptom I have seen once in the case of a man who was squeezed between the bumpers of two cars. I have no time to enter further into the discussion of this symptom other than to say that a history of its presence invariably indicates that the accident was a severe one.

Let us now take up group second, which, I think, we can likewise dispose of briefly. It includes, as you will remember, fractures of the vertebræ and dislocations at the time of the accident. It is exceedingly improbable that gross fracture or displacement will require in courts of justice the services of an expert to establish their existence. The fact and degree of injury are with few exceptions readily recognized even by the layman. In

the so-called "latent fracture of the spine," however,—*i.e.*, cases in which the fracture is attended by little or no displacement,—the symptoms may simulate those of sprain. Time, however, will almost invariably reveal the truth. Consecutive inflammation, necrosis, abscesses tell the tale. Further, the symptoms at the outset are usually, but not always, indicative of injury to the spinal contents,—*i.e.*, we have more or less marked paralysis of motion and sensation and of the bladder and bowels.

The third group, to which I will now pass, includes all those cases which present nervous symptoms, and in which visible evidence of injury may be entirely wanting. The first of its subdivisions deals with cases of actual injury to the spinal contents, such as concussion, hemorrhage into the spinal marrow or its coverings, inflammatory changes, and late on-coming degeneration. The second subdivision deals with those cases in which no other symptoms are present than those of a persistent loss of nervous tone, or, symptoms referable to hysteria, complicated, it may be, by a psychosis. This group is undoubtedly by far the most important. Indeed, many writers appear to restrict the expression "railway spine" to this class of cases. They are sometimes extremely difficult of solution, and they have on more than one occasion given rise to hard-fought legal battles.

To begin with, it is extremely probable that a trauma sufficient to injure the dura mater, especially its internal layer, will also be attended by actual damage of the spinal column,—*e.g.*, fracture, dislocation of the vertebræ, or later on-coming Pott's disease. Lidell,* however, regards meningitis "as among the possible consequences of sprains or wrenches of the vertebral column," and then cites an apochryphal newspaper case as an instance. Leyden† speaks of a case of traumatic pachymeningitis which was hemorrhagic, but the case is valueless, as it was complicated by a fractured skull, and the man had in addition presented symptoms of disease previous to his fall. Leyden, however, describes another case in which, following severe blows upon the back in a railway accident, there were late on-coming symptoms associated, as the autopsy showed, with extensive pathological changes. There was marked myelitis, together with a tumor of the dura and pachymeningitis. He also records that the vertebral bodies in the neighborhood of the disease were eroded.

While traumatic pachymeningitis must be excessively rare, the expert should not maintain that its occurrence is an impossibility. A conceivable method of its occurrence is the following: Hilton declares, as you may know, that the intervertebral cartilages are frequently damaged in injuries of the spine,—*i.e.*, that there is a partial separation of the cartilage from the body of the vertebra. Should this be the case, it is not impossible that inflammation should reach the dura by extension from the posterior vertebral ligament. However, apart from this occurrence being hypothetical, such an instance would come properly under the head of definite injury to the spinal column, and could not be classed as a simple uncomplicated traumatic meningitis. Still, it is not at all improbable that a perimeningitis, an affection which is external to the dura itself, can be set up by blows upon the back insufficient to produce actual disease of the bones.

Passing now to hemorrhage into the substance of the cord or into its membranes,—I mean, of course, gross and not capillary hemorrhage,—we enter a field where no doubt exists as to the possibility of the lesion. That it occurs, no one, not even Page, denies. When present, it produces such marked and grave symptoms that the fact of the patient being badly hurt is very evident. Hemorrhage into the substance of the cord is very frequently fatal, and in the few cases that live the permanent damage done is so unmistakable (wasting and paralysis) that the question for the expert to decide is not so much upon the fact of the injury as upon its extent, and even upon this latter point no great variance of opinion, I take it, would be likely to obtain.

As regards hemorrhage into the membranes, the gravity of the symptoms again places the average case beyond the range of question. It is only in those cases, again, in which a partial recovery takes place,—*i.e.*, in which the hemorrhage has been small in amount, or in which it has been low down in the spinal canal,—that a legitimate discussion can arise as to the permanent injury. Regarding the method of study and the duty of the expert, I will defer my remarks until we have reached a later stage of the subject.

We next come to the subject of true spinal

* Lidell, "Sprains, Twists, and Wrenches of the Back" ("International Encyclopædia of Surgery," vol. iv. p. 688).

† Leyden, *Klinik der Rückenmarkskr.*, vol. ii. 406; also *Archiv f. Psychiatrie*, etc., 1878, vol. viii. p. 31.

concussion. I say *true* spinal concussion because in many cases of railway spine, which are sometimes loosely spoken of as cases of spinal concussion, no concussion has at any time existed. For the benefit of our legal friends, let me say that by concussion of the brain or cord, we mean that the part which has received a blow suffers a loss or perversion of function, which is of greater or lesser duration. The change is sometimes accompanied by slight structural change, such as the escape of blood from the capillary vessels,—*i.e.*, by minute hemorrhages. Very often, however, no visible change can be detected, and the alteration, whatever it may be, is molecular, and has been compared by one of our writers to the loss of its properties by a magnet when it is struck a blow.

Concussion of the spinal cord, unlike the other conditions I have mentioned, has, on the one hand, been freely and excessively admitted, notably by Erichsen;* and, on the other hand, has been as frantically and absolutely denied, notably by Page.† Let us see what one of our own clear-headed writers, a surgeon, has to say on the subject. Dr. William Hunt,‡ after referring to the well-known anatomical relations of the spinal marrow, and showing how well the latter is protected by fat, connective tissue, and its various membranes, proceeds to discuss the subject as follows: "Clinical observation is, I think, of far greater value in determining questions of the kind than any theory, however strongly supported by anatomical facts. Does transient concussion of the spine occur as transient concussion of the brain occurs? Page, if I understand him, says not. In his work on "Injuries of the Spine and Spinal Cord" (London, 1883), in criticising a well-known case as to the claim of persistent paraplegia without discoverable lesion, he says: 'We italicize the word persistent, for simple concussion of the brain may give rise to a transient unconsciousness, and, if the analogy holds good, concussion of the spine should *per se* produce a transient paraplegia. *We know of no case, nor can we discover the history of any case, where this has happened.*'

"I italicize the last sentence. In 1881 a boy came under my care who was shot in the back three inches to the right of the third or fourth dorsal vertebra. He at once had characteristic symptoms in the legs of being wounded in the spine in such a way as to affect the cord somehow. There was partial paraplegia, with pains in both limbs. Under rest these symptoms soon disappeared. In a few days I made a deep incision and removed some clothing and fragments of bone, and then from the depths of the spinal gutter I took a large conical ball which was resting against the bony bridge of a vertebra. The boy recovered rapidly. I saw him some months afterwards, perfectly well.

"Surely this was a case of spinal shock or concussion with transient paraplegia, and the cause of it could have been nothing else than the impact of the ball against the column, producing vibratory jar sufficient to affect the cord. The immediate symptoms and the rapid and complete recovery are, in my opinion, inconsistent with any theory of congestion or pronounced lesion of the medulla."

Another case recorded by Dr. Hunt was that of a man who, falling from a considerable height, landed on his feet. The force of the impact was sufficient to fracture both calcanea. In addition there was paralysis of the legs and loss of control over the bladder and bowels. The man recovered, the spinal symptoms improving along with the fractures.

As a third instance, the doctor cites the well-known case of President Garfield, in whom a bullet struck the spinal column, concussed the cord, and produced temporary loss of power in the legs.

These cases, then, constitute undoubted and undeniable concussion. However, from the further reading of Dr. Hunt's article, I infer that he limits the effect of concussion to *transient* loss or perversion of function, and that he is indisposed, to say the least, to admit any degree of permanence or chronicity or late appearance of symptoms.

The following case from my own experience certainly proves that symptoms accompanying shock may persist indefinitely: I was called one Sunday evening to a man 45 years old, who, while in a condition of average health, had fallen down a narrow and crooked stairway. Though not intoxicated, he admitted that he had been drinking. He fell backward in such a way that he landed upon the last step, striking it almost vertically with the shoulders and back of the neck. When I saw him he was much shocked and depressed and unable to move hand or foot, and his pupils were dilated. The next day I saw

* Erichsen, "On Concussion of the Spine." New York, 1882.

† Page, "Injuries of the Spine and Spinal Cord." London, 1883.

‡ Hunt, "Concussion of the Spine," Pepper's System, vol. v. p. 913.

him again, and he was still completely paralyzed in both arms and legs. There was decided impairment of sensation, though it was nowhere lost. Respiration appeared to be almost entirely diaphragmatic. There was retention of urine, and the bowels were obstinately constipated. There was at no time any vomiting. Under rest and various simple measures the man made an almost complete recovery. I say *almost*, because just here lies the point of special interest. At the end of six weeks he had so far recovered the use of his legs as to be able to leave his bed, and, to a certain extent, had regained power in his arms. The deltoids, however, were as yet almost completely paralyzed, and there was decided weakness of almost all the other arm muscles, especially of the extensors of the forearms. Sensation had become normal. He remained under my care for several weeks longer without any improvement in the condition of the arms, and about a year later, when I saw him again, decided wasting had taken place in the paretic muscles.*

Here certainly is a case in which spinal concussion was followed by persistent symptoms, symptoms, too, which are referable to organic changes in the cord. That organic changes of the cord result from blows and shocks to the back is maintained by authorities of no less weight than Spitzka, Gowers, and Edes. Spitzka, for instance, states that both in posterior sclerosis and in disseminated sclerosis the origin of the trouble can be traced in individual cases to shock and injury. Speaking particularly of disseminated sclerosis, Spitzka† says: "Railway spine is undoubtedly the mask of a disseminated inflammatory trouble in a number of cases." Edes‡ has called attention to the "somewhat frequent occurrence of degeneration of the postero-lateral columns of the spinal cord in so-called spinal concussion," and has placed on record four cases, one of which he verified by a post-mortem examination. Some of the most valuable testimony, however, is offered by Dr. Gowers,§ who not only places blows upon the back among the causal factors of both lateral and posterior sclerosis, but even maintains that damage to the cord by concussion of the spine, either local or general, is far more common than direct injury,—*i.e.*, injury resulting from fracture or dislocation, or from gunshot wounds or stabs. He divides the cases into three classes: First, "those in which the injury causes immediate and severe paralysis," and here there may be either no discoverable change in the cord or as marked a lesion as myelitis. In the second class he places "those in which there are at first either no symptoms or only trifling disturbance of function, but in which grave symptoms come on a few days or weeks after the injury," and, among others, records the following interesting case: "Thus a lady was severely shaken in a railway collision. She seemed, immediately after the accident, to have suffered no injury, but in a few days paraplegia developed, and from its consequence she died six weeks after the accident. Throughout the dorsal region of the cord I found indications of subacute myelitis, chiefly in the white columns, varying in its extent in different regions, but, in most parts, considerable in the pyramidal tracts."

When these patients survive, recovery is slow, but may ultimately be very great. In other instances it may be limited.

In his third class he places those cases "in which there are no early symptoms, or only slight and transient disturbance, but at the end of one or several months symptoms gradually come on, often such as indicate disease of some definite system of structure of the cord, degenerative in nature." This class, of course, includes the cases of lateral, posterior, and disseminated sclerosis already spoken of. Here is an example: J. D., aged 60, a marine engineer, had ten years ago a severe fall on the deck of a vessel. He fell from a height of ten or twelve feet, landing upon his right hip. The hip was severely injured, but not broken. He was confined to bed for several weeks, after which he was able to get about. His recovery, however, was never complete, and after a while he noticed that walking was becoming more difficult. Gradually he grew worse, and at present his gait is observed to be markedly spastic. The knee-jerks are exaggerated. There is no palsy of sensation. The sphincter of the bladder is paretic. No girdle sensations. No improvement during treatment.

* Since writing the above I have had the opportunity of again examining this man. Five years have now elapsed since his fall, and the palsy of the deltoid and upper-arm muscles persists unchanged. The wasting, however, does not seem as marked.

† Spitzka, "Disseminated Sclerosis," Pepper's System, vol. v. p. 884.

‡ Edes, *Boston Medical and Surgical Journal*, September 21, 1882.

§ Gowers, "Diseases of the Nervous System," Philadelphia, 1888, pp. 434 *et seq.* Also, Duménil et Petil, "Commotion de la Moelle Épinière," *Archives de Neurologie*, 1885, vol. ix. pp. 1, 145, 307.

I will not further take up your time in adducing evidence to prove that the spinal cord may be seriously and permanently damaged by blows or shocks which fail to produce injury of the spinal column. It is indeed, in my mind, no longer a matter of opinion. Neither shall I take up your time by a discussion of the manifold symptoms which these patients present, as such a discussion would not only be necessarily of great length, but would also be excessively technical, and therefore out of place here.

I shall pass at once to the second subdivision of group third of our classification, which deals with cases in which the symptoms point to a persistent loss of nervous tone, or to hysteria, or possibly a form of insanity, and in which no evidence of a structural injury can be found. This class of cases includes some of the most difficult with which the expert has to deal. It is here that he must distinguish between the malingerer and the honest sufferer, for we must admit at the outset that traumatic neurasthenia and traumatic hysteria* are matters of actual occurrence. From one stand-point these cases should hardly be considered under the head of spinal injuries, as they are affections of the nervous system as a whole, and they appear to be a result not only of the mechanical shock, but also of the excessive mental and emotional excitement accompanying the accident. In fact, the term railway brain has been suggested as more suitable to some of these cases. Page† under the head of "Shock to the Nervous System," discusses this condition at some length and tabulates its various stages and symptoms. He defines it as shock of the cranial contents without any structural lesions of importance. At the same time, he admits that "some of the symptoms . . . often remain for years as the sequelæ of severe concussions." He also states that while‡ "the great majority of cases may happily be placed in the stage of convalescence, there are occasional instances of persons who never reach it, having succumbed at an early date to the severity of the shock;" and then he proceeds to cite two fatal cases, one dying on the thirty-seventh day and the other in about five weeks after the accident. In this connection he says, "Though moderate in character, it (the accident) no doubt exercised a very unusual influence in depressing vital powers,—never very strong naturally,—and finally inducing such an amount of nervous exhaustion as to terminate fatally, even though there were no evidences before or after death of physical injury to any one part." The second point, then, that we have to consider is, that the consequences of "shock to the nervous system," or traumatic hysteria, or neurasthenia,—whatever we may choose to call it,—may vary greatly in degree. From a mere temporary fright or nervousness, that passes away in a few hours or days, we may have all grades of functional disturbance up to those that gradually deepen into death. Luckily, however, the latter result is a very rare one. Many, perhaps the majority of cases, undergo more or less marked improvement with the progress of time, though this improvement is rarely, if ever, complete. Convalescence is always gradual, often very prolonged, and often extending over years.

Sometimes, as already hinted, the symptoms are strikingly hysterical. There is unusual mobility of the emotions, globus hystericus, perhaps hysterical palsy, and even hysterical hemianæsthesia. At other times we have more the symptoms of a general depression of nervous tone. The man is irritable, perhaps morose or hypochondriacal, incapable of even slight exertion. He complains of headache, inability to fix the attention, loss of memory, and inordinate susceptibility to the influence of alcoholic stimulants. "He may," as Knapp in his excellent paper puts it,§ "present no objective symptoms, but he remains an utter wreck. There is a general weakening, and a decline from the normal standard in the functions of the central nervous system, especially in the domain of thought, the will, and the emotions." In these cases, too, it should be remembered, a painful condition of the spine, probably neuralgic in character, persists, often for years.

I will not touch upon the various psychoses that occasionally follow trauma, other than to point out that the transition from such a condition of the nervous system as just described to that of actual insanity is merely one of degree. Krafft-Ebing‖ and others¶ admit its actual occurrence.

* Charcot, "Leçons sur les Maladies du Système Nerveux," vol. iii. Also, Putnam, *Boston Medical and Surgical Journal*, September, 1883, p. 217. Also, Walton, *Archives of Medicine*, August, 1883.

† *Loc. cit.*, pp. 170 and 171.

‡ *Loc. cit.*, pp. 167 *et seq.*

§ Knapp, *Boston Medical and Surgical Journal*, November, 1888. (Reprint.)

‖ Krafft-Ebing, "Lehrbuch der Psychiatrie," Stuttgart, 1888, p. 184.

¶ Hartmann, *Archiv für Psychiatrie und Nervenkrankheiten*, 1884, vol xv. p. 98.

In order to impress upon you more fully the condition of patients who are suffering from a nervous shock, I will cite the following cases from my own experience:

An estimable middle-aged lady, a fine, plump, and wholesome-looking matron, whom I had known for a number of years, had been, with the exception of occasional slight muscular rheumatism, in a condition of perfect health. While travelling in a railway train, about a year ago, she left her seat in order to alight at a station which the train was approaching. It so happened, through some mistake of the railroad employés, that a freight-engine was standing on the main track. A collision of course was the result, and the lady was thrown violently upon her back on the floor of the car. She was somewhat shocked and considerably bruised. Intense aching and soreness of the back followed, and from having been a self-possessed and common-sense sort of woman, she became nervous, depressed, and at times burst into causeless tears. She seemed to worry and yet to have no special cause for it. Her facial expression, too, changed from that of quiet and contentment to that of trouble and concern. In addition, she looked decidedly older. Improvement, however, gradually set in some eight months after the accident, and now, at the end of a year, she is almost well. Her nervousness has almost altogether disappeared, and her spirits are again up to the normal. All this has happened without the balm of damages. Her backache, however, though much lessened, still persists, but not steadily, being only troublesome now and then.

This case illustrates, therefore, a mild instance of nervous shock. Here is another in which the symptoms were more pronounced and of longer duration.

A perfectly healthy woman, 30 years old, had all of her life been accustomed to hard work, had borne a number of healthy children, and at times during the illness of her husband served truck with a horse and wagon, being out for many hours at a time, exposed to all sorts of weather, and climbing in and out of her wagon many times a day. One day, while four and a half months pregnant, she boarded a two-horse bobtail car. She took a seat near the back part of the car, sitting rather forward, so that her back was some distance from the back of the car-seat. The driver left his horses, still going at the same rate of speed, and entered the car to collect the fares. While thus engaged a collision occurred between the car and an odorless excavating truck. My patient felt a sudden jar, was thrown violently against the back of the seat, her back striking the back of the car-seat and her head the framework between two adjacent windows. She remembers nothing of what occurred immediately following, other than seeing some of the passengers reentering the car. She felt faint and sick, and took the next car home. Could not eat any supper, and could not sleep that night because of pain in the back. Got up the next morning, but suffered so much that she was obliged to go back to bed. Was in bed for three days. Had also at this time considerable flow per vaginam. On the third day commenced passing her water every few minutes. Was obliged to get up for this purpose frequently at night. This has continued ever since. Her condition remained unchanged for a month, when on attempting some unusual exertion she became unconscious. On coming to, she discovered that she had lost power in the left arm and leg. These members were completely paralyzed. She improved, however, and at the end of a week was again able to walk, though she did so with difficulty. Three months later, when I first saw her, the left leg was still weak. I made at the time the following note: On stripping the patient and examining the spine two very tender spots are detected, one over the seventh cervical vertebra and the other over the sixth or seventh dorsal. Lesser but decidedly painful areas are discovered in the lumbar and sacral regions. Grip of the left hand decidedly diminished. Left leg drags perceptibly in walking. Stands well on the right foot, but is in danger of falling when attempting to stand on the left. Knee-jerk excessive on the right side and diminished on the left. Complains that the left leg and arm feel dead and numb, especially the leg. When examined by the æsthesiometer, the responses are decidedly slow. On the right side they are immediate. On the left side she separates the points either not at all or with difficulty. On the hand the points must be two and a half to three inches apart before she recognizes a dual impression. On the arm she cannot separate them at all. On the leg there is a patch of absolute anæsthesia on the outer aspect of the calf. On other portions of the limb sensibility is merely diminished. On the right side the responses are everywhere normal. At the time of this examination she was eight and a half months pregnant.

The patient continued in this state, without any change of her symptoms beyond some increase in the weakness of the left side, until

she fell in labor. The latter, which was very unusual in her case, was very prolonged, lasting three days. It was terminated, however, without the use of instruments. On the second day following she commenced to have headache and a diffused pain in the entire left side. This headache persisted for some six weeks. Two weeks after her accouchement she left her bed. Walked, however, with great difficulty, and on one day the loss of power on the left side was again all but complete.

Four weeks afterwards she was examined again. There was continual aching all down the spine, but the tender spots before noticed had disappeared. She was still compelled to pass her water very frequently, though she thought that she was a little better in this respect. The left hand, too, had gained somewhat in strength. The right leg now was weak, more so, indeed, than the left. The knee-jerk was much diminished on both sides. Cutaneous sensibility of left side was improved, though still below normal.

For one month following she continued to improve, the improvement, especially in walking, being very marked. Then she reached a stand-still, the backache, headache, weakness, and numbness being now better and now worse. Her general nutrition and color excepted, no decided change in her condition could be noted several months later. At the present time, a year and a quarter after the accident, her symptoms persist, though in a lessened degree, especially the headache, backache, and weakness in the knees.

I should state that in this case, though it involved a suit for damages, the period of the improvement bore no relation to the time of the trial, the maximum improvement taking place some six months previous to the latter.

In this case, then, the nervous shock, influenced no doubt also by the fact that it occurred in a pregnant woman, resulted in a pronounced hysteria, and that, too, in a woman who had previously had a robust and fibrous temperament.

It will lend additional interest to this case when I tell you that the child born of this woman has epilepsy. The seizures commenced shortly after birth, and continue now at irregular intervals. They appear to be in every way typical, and average three or four a week. No member of her family has ever suffered from this disease. Surely this looks like a case of maternal impression, and suggests to us how profound the shock must have been to the nervous system of the mother.

Another case is that of a man, and is particularly interesting as being the outcome of a series of accidents. The following is his history: M. C. B., aged 47, married, and at present is an oil distributor in the employ of the Pennsylvania Railroad. Was born in Missouri. Claims to have come from healthy stock. Was perfectly well up to eighteen years of age. At that time contracted gonorrhœa and a venereal sore. Did not suffer from any sequelæ that he can remember. Married later, and has had a family of apparently healthy children. Wife has had no miscarriages. One child died shortly after birth, said to have been a "blue baby."

His own health had always been good, and for this statement I have not only his own assurance, but also that of his wife. In 1877 he was injured as follows:

He was working as a brakeman on the Pennsylvania Railroad, and while coupling cars his body was caught between the "niggerheads" of the tank of the engine and of the cabin. He was badly squeezed from before backward, so severely, indeed, that it seemed to him as though the edges of his lower ribs touched his backbone. He was in this position fully five minutes. He was not senseless when he was extricated, but would certainly have fallen to the ground had he not been caught by his mates. He was taken to the University Hospital, where he remained upward of five weeks. At the end of the fourth week he could stand, and before the end of the fifth he was able to walk around the hospital. Thinks now that he could have walked earlier had he tried. He was, however, exceedingly nervous, and on returning to his position he found that the sight of a passing train made him giddy. His feet and legs were also exceedingly cold. He also had backache, headache, and slept badly. He continued without marked change in his condition until 1880, when he met with another accident. This time he was struck in the back by the bumper and iron step of a cabin, the latter moving, he asserts, at a rapid rate. He was knocked senseless and received a scalp wound, but, strange to say, returned to his work two days afterwards. His backache, headache, nervousness, and giddiness all increased in severity. He also noticed that he could not see as well as before. In 1881 he had still another accident, and this seems to have been the last straw that broke the camel's back. While coupling cars three fingers of the left hand were caught and mashed. Two of them required partial amputation. His ner-

vousness, sleeplessness, and pain in the back were now worse than ever. The backache was at times so bad as to absolutely interfere with his work. He was now an oil distributor. Some six months after this accident, being on night duty, he returned home in the morning and, after a slight altercation with his son, went to bed. After sleeping about an hour he awoke screaming with all his might. He screamed for several minutes, was perfectly conscious, but was utterly unable to control himself. After this his general condition remained about the same until 1887, when he had another screaming spell. These spells recurred and soon became very frequent. Sometimes he had as many as two spells a day, and they occurred indifferently during his waking or sleeping hours. He was always conscious during the attack. The spells have been less frequent of late. Before the attack comes on he feels as though something were being pumped up his back, and "by the time it reaches his throat it bursts" and he *must* scream; there is no help for it. After the spell is over he feels as though something were running down through his trunk, the sensation being very much like that which is conveyed to "the hand in grasping a garden-hose through which water is running."

In addition to these undoubtedly hysterical symptoms, the man has still a painful back, and which is very tender on pressure. Headache is also a frequent symptom. Clavus hystericus is sometimes present. He says that at times his calves are anæsthetic, but this, if true, is a variable symptom. He is also constantly complaining of new and strange pains and aches.

There is no contraction of the field of vision, but Dr. de Schweinitz reports that the optic nerves are gray. There is also a very slight inequality of the pupils, but this appears to be normal. Response to light and accommodation are also normal.

This case, then, appears to be one, all things considered, of very pronounced hysteria and neurasthenia, and undoubtedly the various traumas received were the essential causes.

Before closing, let me impress upon you again that the classification of spinal injuries I have proposed is purely arbitrary, and, while most cases permit of being classified under some special head, others may be found in which the symptoms are referable to two or more divisions of our classification. It is possible, for instance, that a sprain of the back be accompanied by damage of the spinal contents or *vice versa*, or that either of these conditions be accompanied by hysteria. The mere presence of sprain of the back should not, therefore, lead the expert to ignore the possibility of injury to the cord, nor, on the other hand, should the presence of undoubted hysterical symptoms lead the expert to ignore the possibility of actual structural change. However, it is the duty of the expert, whenever possible, to separate the organic affections from the purely neurasthenic or hysterical. The distinction is a vital one, inasmuch as it affects the future of the case and of course the result of the suit for damages. As regards the prognosis of hysteria, its degree and the concomitance or non-concomitance of neurasthenia are to be considered. Simple uncomplicated hysteria, it must be remembered, offers, other things equal, far better chances of recovery than simple neurasthenia, especially when the latter is profound. As a matter of fact, however, hysteria and neurasthenia so often go hand-in-hand that a sharply-drawn distinction cannot always be made. Again, neurasthenia is to be considered, other things equal, as the more serious the more pronounced the mental symptoms, remembering that actual insanity, though rarely, may supervene.

Lastly, regarding malingerers,—that is, those whose affection is neither hysterical, neurasthenic, nor organic,—the expert must rely largely upon his mother-wit for the detection of the fraud. Distinction between actual and simulated organic disease can, as a rule, be readily made, though between simulated disease and hysteria the line is sometimes hard to draw. Hysterical functional disease is, however, *real as far as the patient is concerned*, and needs *no active effort* on the part of the patient to maintain its outward manifestations, while in the malingerer a *constant effort* is required, and if closely watched he may be caught napping. Further, there is a marked difference, as a rule, in the mental attitude. The hysterical patient is generally emotional in the extreme, and presents such typical symptoms as globus hystericus, clavus, or even typical hysterical hemianæsthesia. The malingerer is, on the other hand, a complacent, often argumentative, liar, and occasionally displays a superficial familiarity with technical terms, as though he had studied his part. Not infrequently he contradicts himself, and can, by leading questions, be trapped into the grossest absurdities. Neurasthenia, as already stated, is so frequently wrapped up with hysteria that here another

point of difference can be sought for. The malingerer, of course, bears upon his face, other things equal, the aspect of health; shows no change in his pulse or heart's action; there is nowhere any evidence of vaso-motor relaxation, no cold and bluish extremities, no change in the reflexes, etc.

This brief summary would be incomplete if I did not point out the danger to which the expert is exposed by the exaggerations, voluntary or involuntary, of people who are really hurt or who honestly believe that they have been hurt. The investigation must be both detailed and exhaustive, and the patient should be encouraged to talk, and talk freely, while the doctor should be a quiet listener, carefully weighing and recording each statement. After all, it will be his common sense and native acumen which will guide him in his conclusions.

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*SPINAL LOCALIZATION IN ITS PRACTICAL RELATIONS.**

By Charles K. Mills, M.D.†

PART I.—GENERAL CONSIDERATIONS.

SPINAL localization, particularly in its practical relations to surgical procedure, is one of the topics of the hour which has not yet been fully discussed in any of our Philadelphia medical societies. Although its proportions do not approach those of cerebral localization, the subject has sufficient literature and interest to allow of lengthy treatment and open a wide field of discussion; but it will be necessary for me to be brief in order to cover the ground.

Spinal trephining is by no means a recent operation; it has an ancient history both as regards suggestion and actual performance. Horsley‡ speaks of it as known to surgery since the time of Heister (1757), but Lidell attributes the suggestion to Paulus Ægineta. An operation of M. Louis, performed in 1762, is sometimes referred to as the first instance of spinal resection, but, as stated by Ashhurst and Lidell,§ it was not a resection at all, but merely an extracting of some loose fragments of bone on the fifth day in a case of gunshot fracture involving a dorsal vertebra, an operation which was performed twenty-four times during our civil war with satisfactory results. Genuine spinal trephining was first performed by Henry Cline, June 16, 1814, for fracture of the twelfth dorsal vertebra. Horsley, writing last year, cites Erichsen as stating that it has been performed in all about thirty times; but Ashhurst, about the same time or earlier, tabulated fifty cases. Of these fifty cases, thirty-three died, seven were not benefited, four were relieved, two recovered, and in four the result was unknown. I have collected fifteen cases in addition to those in Dr. Ashhurst's table, and doubtless a few others have escaped me. In addition to these I have personal knowledge of three other cases in which spinal trephining has been performed, but in which the accounts of the operations have not yet been published.

While we have here, however, a record of sixty-eight cases in which the operation of spinal trephining has been performed, a close study of the details of these cases shows that very few operations were guided strictly by the rules and principles of spinal localization. External pointers, as deformity, depression, or swelling, were commonly present. Gowers and Horsley's case of spinal tumor is perhaps the most brilliant example of localization, and vies with the cases of Macewen and Abbe in brilliancy of result.

Many experimental investigations of the functions of the spinal cord have been performed by Brown-Séquard and a host of physiologists, but the first experiments to determine the exact functions of the horns of the cord at various levels were those of Ferrier and Yeo,‖ in 1880. Pathological observations had been made prior to this time and have been since, and are largely confirmatory of the results obtained by these investigators. In 1884, Starr¶ collected most of the work on this subject up to that date. Ross** has worked in the field of sensory localization to good purpose, contributing an excellent paper on the "Segmental Distribution of Sensory Disorders." Much material of a highly practical character will be found embodied in Gowers's "Manual of Diseases of the Nervous System" and in Seguin's contributions to localization in the "American System of Practical Medicine." Recent valuable papers, partly neurological and partly surgical, on spinal

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‡ *Med.-Chir. Trans.*, vol. lxxi., Lond., 1888, p. 383.

§ "Intern. Encycl. Surg.," vol. iv.

‖ "Proc. Royal Soc.," No. 212, and *Brain*, July, 1881, vol. iv. p. 223.

¶ *Am. Journ. Neur. and Psych.*, vols. ii. and iii. p. 443.

** *Brain*, January, 1888.

localization are those of Thorburn.* This writer records nineteen cases of injury to the cervical region of the spinal cord, studied with the view of determining the exact spinal localization as indicated by such injuries. He has also contributed a valuable article on injuries of the cauda equina. Osler† has published a brief paper on lesions of the conus medullaris and cauda equina, and on the situation of the ano-vesical centre in man. Other papers on allied topics have been contributed by Bernhardt, Oppenheim, and Kirchoff, who are cited by Osler.

It is important to closely study every case of supposed spinal focal lesion, whether from injury or disease. Reports of such cases, particularly when accompanied by autopsies, will continue to be useful until a number of points now obscure become clear; but even without autopsies they may be of great value if minute and accurate. Some of the papers on cerebral localization, most valuable because of their suggestiveness, have been purely clinical and largely speculative; as, for example, some of Hughlings Jackson's and Broadbent's on affections of speech. One of the most instructive articles on spinal localization, that of Remak‡ on the localization of the lesion in anterior poliomyelitis is based entirely on clinical cases. From a careful study of many cases, Remak shows how various groups of muscles are frequently affected together, and, reasoning back to the lesion, concludes that disease in a certain segment or adjoining segments of the spinal cord will produce paralysis in a group of muscles which act together. He made some highly-interesting observations with reference to palsies of particular muscles or muscular groups. These investigations will be referred to again later in this paper.

NOTES OF CASES OF SPINAL FOCAL LESION, WITH AUTOPSIES.

Let me first refer briefly to a few cases of spinal focal lesion, with autopsies, occurring in my own experience, briefly commenting on them with reference to the possibility of successful operation.

While it has been my fortune to see a large number of patients suffering from intraspinal focal lesions, I have made comparatively few autopsies on such cases. I find among my notes, however, records of six post-mortem examinations, three of which have been reported. Of the six cases, two and perhaps three might have been trephined with some show of success. One of these was an injury in the lower dorsal region in which the lamina of the twelfth dorsal vertebra had been broken and driven inwards, a case similar to one of the operative cases reported by Macewen, similar even to the vertebra injured. A bony mass was present about half an inch long by one-quarter of an inch wide in the external layer of the dura, just at the lower end of the spinal cord. This mass, which was like the bony concretions sometimes found in the falx cerebri, could have been removed by operation, after elevation of the depressed and broken spinal arch.

In another case operation might have been of benefit if performed immediately after the accident. The most important features of this and the first case were as follows: A history of severe injury to the middle and lower spine; this was followed at once by pains in the back and limbs, spinal rigidity, anæsthesia, and paralysis of the lower limbs and of the bladder and bowels. The condition remaining was one of motor paralysis, wasting, contraction, depressed electro-contractility, coldness and lividity, peculiar zones of anæsthesia, and abolition of the tendon reflexes in anæsthetic regions. In this second case, at the upper edge of the twelfth dorsal vertebra adhesions were found which continued down the canal for three or more inches. At the commencement of the cauda equina, which was about the centre of the adhesion, caries of the first lumbar vertebra was found, the portion involved being the inner surface of the laminæ close to the junction of the body. Above and below the caries the adhesions were most persistent. The conus medullaris, cauda equina, and membranes were a glued mass. For an inch or two more above and below this the membrane was considerably thickened, a deposit being present of a calcareous nature.

In a case of caries and dural tumor, of which I exhibit specimens, the patient might perhaps have been benefited by relief of pressure. This patient, a man 72 years old, was admitted to the Philadelphia Hospital with a history that he had been lame in his left leg, the result of a fall, since he was ten years old. No relatives or friends could be found, and, as his mind seemed somewhat clouded, it was difficult to obtain from him any statement of value as to his past condition. It was learned, however, that three

* *Brain*, January, 1887; January, 1888; and October, 1888.

† *Med. News*, vol. liii., December 15, 1888.

‡ *Arch. f. Psych.*, ix.

or four months before admission he had had an attack in which he suffered from diarrhœa and fever; that he became weak in both legs, but largely recovered power in them, until four weeks before admission, when he became a complete paraplegic.

He had no paralysis of the face and arms. His left leg was shorter than his right, and was considerably atrophied, measurements showing about an inch difference between the limbs. Motor paralysis was complete in both legs, but he had control over the bowels and bladder. Tactile sensation was diminished in both legs, and on his trunk to a line on a level with the third dorsal spine behind and the fourth rib in front. Pain and temperature sense were preserved. He complained of a belt-like feeling around the waist at the highest level of the anæsthesia. He had a sacro-ischiatic bedsore nearly central in position. He died a few days after admission to the hospital. The brain showed no gross lesion. On opening the spinal canal, when the line of the fourth dorsal vertebra was reached the dura mater was found to be strongly adherent, requiring dissection, and a small tumor or enlargement was attached outside of this membrane on the lateral aspect of the cord. It was about three-quarters of an inch in length and one-third of an inch in thickness. Possibly the cord was slightly diminished in general bulk from compression at this point, but it was not abnormal in consistency, and no hemorrhage nor inflammation were present. The body of the fourth dorsal vertebra throughout was found to be eroded and in a carious condition. For a little distance below the periosteum was detached and the bone partly exposed. The disease of the bone had destroyed nearly the whole of the interior of the body of the vertebra. Below the index-finger could be passed into the cavity, while above was a shell or bridge of bone. The vertebra was not displaced. The intervertebral cartilage between the fourth and fifth dorsal vertebræ was also necrosed. The upper lobe of the right lung was strongly adherent to the bodies of the third, fourth, and fifth dorsal vertebræ. A communication existed between the space in the decayed vertebra and the pleural sac.

In two cases of syphilomata of the dura mater of the cervical region, operation would have been useless from the nature, extent, and high level of the lesions.

The sixth case was one of hemorrhage into the cauda equina, the symptoms being similar to those in the second case referred to above. The question whether in such a case early trephining at the lowest point of the lumbar region would have been feasible, and would have offered any prospect of relief, will be considered later.

SPINAL OPERATIONS GUIDED BY LOCALIZATION. SPINAL FRACTURES AND DISLOCATIONS.

Trephining the spine in whole or in part, guided by the rules of localization, may be resorted to for fracture, dislocation, tumor (including certain hypertrophic inflammatory pachymeningeal lesions), hemorrhage, abscess, caries, and neuralgia.

Page* has said, with reference to fracture and dislocation, that trephining of the spinal column can be of no advantage; but this is certainly far from being true of the former, and even in some cases of dislocation the operation may be of assistance to the surgeon in bringing about reduction. Other eminent surgeons, as Lidell and Ashhurst, hold that more lives are lost than saved by interference. Whether operation in cases of fracture and dislocation will or will not succeed is largely a question of exact diagnosis of the extent and location of the injuries. When, for instance, it can be demonstrated by close study that the brunt of the injury has been borne by the spines and laminæ, operation should be at once performed, and even when in doubt it is better to explore rather than to hesitate in the face of paralysis or death.

An interesting recent successful case is one performed in 1887 by an English surgeon, Mr. Jones, reported by Thorburn in *Brain* for January, 1888, and also later, as to result, in the *British Medical Journal* for October 20, 1888.

The patient, a collier, aged 15, was injured by a heavy stone striking him on the shoulders and back. He received a fracture of his right femur, and was also instantly rendered paraplegic. The limbs presented a position of partial paralysis, with wasting, lost faradic contractility, absent knee-jerk, changed skin reflexes, paralysis of the bladder, cystitis, and priapism. Sensation was blunted or lost over special nerve areas in the lower limbs. The third lumbar spine presented a prominence. To the left and above was another bony prominence. Trephining was performed. The detached spine

* Heath's "Directory of Surgery," p. 134, quoted by Horsley.

of the second lumbar vertebra was removed, and showed a gap between the arches of the second and third, filled with dense cicatricial tissue. By means of bone forceps the arch of the second lumbar was now almost entirely removed, exposing the membranes of the cord, which had obviously been compressed by it. Around these membranes was also cicatricial tissue, which was not interfered with. Twenty months after the operation the patient was exhibited at the Manchester Medical Society by Mr. Jones. The condition of the patient was then very satisfactory. Sensation was normal in both legs, which were, however, much wasted, and there was evidence of degeneration in the muscles of the left limb. The movements of the knee-joint were normal, but those of the ankle were limited to extension, flexion being practically abolished. He had perfect control over his bladder, and could urinate normally. He was able to walk three miles with short intervals for rest. When standing he was obliged to lean on some support unless he bent his knees. He was gradually improving.

An equally interesting, but unsuccessful, case, operated upon by Mr. Hardie, is also reported by Thorburn.* The patient was a man 38 years old. The operation was performed October 26, 1887. The patient had a fall of six feet from a wagon, and immediate paralysis without loss of consciousness followed. He had pain across the shoulders and shooting down the arms to the elbows, and arching backward of the cervical vertebræ. The lower limbs, abdomen, and thorax were absolutely paralyzed. His respiration was diaphragmatic, with sense of dyspnœa and cough. All the muscles of the right upper extremity were paralyzed, and the extrinsic muscles of the right upper limb also paralyzed,—the pectorals, spinati, latissimus. The neck muscles were normal on both sides. The muscles of the left upper extremity escaping paralysis were the biceps, supinatus longus, deltoid, supra-spinatus, and infra-spinatus. Both palpebral fissures and pupils were smaller than usual. Anæsthesia extended as high as the third rib in front and the sixth cervical spine behind; on the right side there was sensation over the region of the deltoid and slightly beyond it, and on the left over the deltoid and down the outer side of the limb to the thumb. No hyperæsthesia was present. Knee-jerk, plantar, cremasteric, gluteal, and epigastric reflexes were absent. The temperature was normal. The urine was retained, the penis turgid. His pulse was 66, with a marked respiratory wave. A vertical incision about four inches long was made over the cervical spines, with its centre opposite the fifth. The muscles being cleared from the laminæ, an interval of about a quarter of an inch was found between the fifth and sixth spines, and the fifth vertebræ appeared to be slightly displaced forward. The laminæ of the fifth and sixth vertebræ were removed by bone forceps and the dura mater exposed, presenting a perfectly normal appearance without any trace of hemorrhage. The patient died the next day. The heart-beats, which were very feeble and infrequent, continued for about ten minutes after respiration had ceased, but stopped at 3.35. The disk between the fifth and sixth cervical vertebræ was found to be ruptured, the former bone projecting very slightly forward. No fracture was discovered. The dura mater was uninjured, but the cord was flattened opposite the seat of the injury, and was much contused for about an inch above and below, containing hemorrhage in its substance and in the central canal; elsewhere its structure was normal.

In lesions of the cauda equina which cause compression, whether fracture, hemorrhage, or abscess, but particularly fracture, trephining is undoubtedly indicated in most cases, and in some, with our present light, it is imperatively demanded. This view I take from personal experience of cases with autopsies, and of others which I have studied clinically. The same view is strongly taken by Thorburn,† who shows that some of the objections which are valid against trephining higher in the vertebral column, do not apply so fully to injuries of the cauda equina. He advocates trephining in injuries of the cauda equina, always bearing in mind the following conditions:

"1. We must be sure of the localization.

"2. We should, if possible, wait for a reasonable period, say six weeks, before operating, and should then do so only if the patient shows no signs of spontaneous recovery.

"3. Should the paralysis of the bladder be early followed by severe cystitis, and should we suspect secondary renal troubles, what are we to do? On the one hand, not to operate probably means death; and on the other, the risk of operation is enormously increased. Further

* *Brain*, October, 1888.

† *Brain*, January, 1888, p. 406.

experience may enable us to settle this problem, but at present the outlook appears to be sufficiently gloomy. A third course has suggested itself to me, but I have had no opportunity to test its results,—viz., to drain the bladder by means of a suprapubic cystotomy. The object of draining the bladder is obvious, and the suprapubic appears preferable to the perineal route, because we are thereby enabled to construct our fistula through parts which are not anæsthetic and predisposed to slough. If we could thus prolong life for a time, we might afterwards proceed to operate upon the spinal column with fair hopes of a successful result, and encouraged by the great improvement effected in the case of R. M. C., and the cure obtained in a case recently published by Lauenstein, in which there was complete paralysis of the lower limbs, etc., from a dislocation of the twelfth dorsal vertebra, and where a perfect cure resulted from the removal of the misplaced arches. It would appear, therefore, that whether 'trephining the spine' is or is not justifiable in cases of injury of the cord, it is certainly the proper treatment to pursue in those of the cauda equina, and hence the importance in such cases of making an exact diagnosis."

SPINAL TUMORS.

In most cases of tumor of the spinal cord or its envelopes, operation would not for several reasons be completely successful. It would, however, be entirely successful in some, and I have come to the conclusion that almost all cases should be trephined for the chance of success, or to relieve pain and diminish the effects of pressure. When the growths spring from the bone or dura mater the chances are, of course, the best.

Even if the neoplasm is situated in front, or surrounds a large part of the cord, trephining might be successful, for during operation the lateral and even the anterior aspects of the cord can be explored, the latter certainly by cutting one or two spinal nerves. This can be seen by examining the cord in position in a fresh specimen. With a little care, using a blunt hook, the cord can be rolled and lifted so as to examine it on almost every side in position. Erosion or caries, with perforation of the body of the vertebræ, can also be determined by careful explorations with properly-curved probes thrust around and beneath the exposed cord.

Three operations for spinal tumor, all of them successful, have been reported by Macewen,* Gowers and Horsley,† and Abbe.‡ The case of Macewen was a boy, 9 years of age. The operation was performed May 9, 1883. The main symptoms were complete sensory and motor paraplegia, with incontinence of urine and fæces. Angular curvature was present, most marked between the fifth and seventh dorsal vertebræ. The laminæ of the fifth, sixth, and seventh dorsal vertebræ were removed. A fibrous neoplasm attached to the theca, between it and the bone, was dissected off. After this, cord pulsations, which had been absent, returned. Twenty-four hours after the removal of the pressure the limbs had lost their livid color and were distinctly warmer; the spastic rigidity had greatly lessened. The sense of tickling had returned to the soles, and that of touch had improved. After eight days the first return of movement was observed; soon after he had perfect control over his sphincters. Six months subsequently he was able to go about without support, and five years afterwards he walked three miles to pay the doctor a visit. He attended school regularly, joined in all the games, including foot-ball, and said he felt quite strong. Charcot has pointed out the occurrence of these connective tissues in cases of caries, and one of the specimens exhibited in connection with this paper is a tumor of this character.

The case of Gowers and Horsley was a man, aged 42 years, and the operation was performed June 9, 1887. He had suffered for three years from localized pain beneath the left scapula. The pain varied considerably. He showed great mental irritability and hypochondria. Four months before operation first the left and then the right leg became weak, and gradually there was complete paraplegia. He had motor and sensory paralysis up to the level of the sixth or seventh dorsal nerves, with intense spasm in the legs, with foot clonus and rectus clonus. Urine was retained and he had some cystitis. He suffered with severe pain around the trunk, which was greater on the left side, and increased to agony by any movement. Caries and syphilitic disease of the spine were excluded. The spines and laminæ of the fifth, fourth, and third dorsal vertebræ were removed, and a fibro-myxoma, about the size of a filbert, was discovered and removed. Mr. Horsley reported, January 4, 1888, that

* *Lancet*, August 11, 1888.

† *Brit. Med. Journ.*, January 28 and June 16, 1888, and *Med.-Chir. Trans.*, vol. lxxi.

‡ *N. Y. Med. Rec.*, February 9, 1889.

the pain was for a time slightly relieved, but again and again returned with great severity, and the power of motion was only slowly and intermittently regained during the first three or four weeks. The local pain gradually diminished and motor power gradually returned. The surgical result was excellent. He reported, June 12, 1888, that the patient had lost the agonizing pain, and at the same time gradually recovered motor and sensory power, as well as the control of the bladder and rectum. He remains in perfect health.

One of the most recent contributions to the subject of spinal trephining, that of Dr. Robert Abbe,* is a case of extradural tumor of the spine, with complete paraplegia, in which the operation resulted in recovery. The patient was a young man, aged 22 years, and the operation was performed on May 26, 1888. The first symptom was pain in the back; swelling of the soft parts two inches wide by three long. There was disturbance of tactile sensibility in both legs, followed by gradually-increasing paresis. The line of hyperæsthesia was between the anæsthesia and the normal skin. He had uncontrollable twitching of the legs, which were cyanotic when dependent. The spine became rigid and very painful; the hyperæsthesia, anæsthesia, and paralysis deepened. He suffered with girdle pains about the limiting line of the disease. He had incontinence of urine and fæces. Active hectic was present. The spines and laminæ of the eighth, ninth, and tenth vertebræ were cut away, and a dense mass of tissue and desiccated pus was removed by scraping. Sensation began to return on the eighth day; in the fourth week he began to move his left leg and toes on the right. The fever and pain disappeared, and in six weeks he moved both legs. He improved continuously, and in four and a half months he moved, pushing a chair. He was exhibited to the Surgical Society of New York eight months after the operation, when he walked well without support and was stout and heavy.

In the disease described by Charcot and Jeffroy as hypertrophic internal pachymeningitis, operation for the removal of the lesion might in some cases prove successful. Usually after the disease has existed some time the lesion becomes practically a fusiform tumor of considerable size. It might be objected, perhaps, that the disease is inflammatory in character; but, on the other hand, it is chronic, often of slow development, and sometimes almost stationary. No harm could result in a disease always one of great suffering and eventually fatal, and great relief and possibly a cure might sometimes be effected.

* *N. Y. Med. Journ.*, November 24, 1888, and *N. Y. Med. Rec.*, February 9, 1889.

INTRASPINAL HEMORRHAGE.

As within the cranium, so within the spinal canal, hemorrhage may be of four varieties as to location,—namely, (1) supradural or extrameningeal; (2) subdural; (3) subarachnoid or subspinal; and (4) intraspinal,—that is, into the spinal cord itself. The hemorrhage may be posterior, anterior or lateral, or antero-lateral, postero-lateral, etc.; but commonly it will surround the cord; if intraspinal, it may be into the anterior or posterior horns, the central gray matter, or columns, but usually it will be diffused through the cord. With reference to the question of trephining it is only practical to consider supradural or subdural hemorrhage.

A net-work of large blood-vessels lies between the dura mater and spinal bone, and from this supradural spinal hemorrhage usually occurs. Such an extravasation may be minute, of moderate size, or very extensive. The symptoms of supradural hemorrhage will vary, of course, with its location and extent. The history is generally of a sudden attack, often after a fall or exhibition of violence of some kind.

Hemorrhage between the dura mater and the bone probably usually finds its way down the whole length of the spinal canal to the beginning of the sacral canal. The space surrounding the cauda equina in the lumbar canal is comparatively large, and the much-narrowed sacral canal is placed at a slightly different level, so that much of the effused blood would probably accumulate in the lumbar region. The fat and connective tissue between the theca and the bone are loosely distributed in the extradural space, and differ considerably in amount in different individuals, so that in some cases more than others much of the blood would be entangled and coagulated in the meshes of the connective tissue and fat near the seat of extravasation. The subdural space, although much smaller than that outside of the dura, is a smooth serous cavity, and therefore much of a hemorrhage at any level would also probably descend into the region of the conus and cauda equina.

I do not know of any reported case in

which trephining has been resorted to simply for the relief of intraspinal hemorrhage. In a case of resection at the Bellevue Hospital, New York, by Dr. Stephen Smith,* it is reported that notwithstanding from eight to twelve ounces of extravasated blood, having a dark color, escaped from the spinal canal after the depressed bone had been extracted, the compression of the cord from extravasated blood and the paraplegia steadily crept upward, and finally caused death by asphyxia. The case is reported to show that the operation of trephining the spinal column or resecting the vertebræ would not relieve the spinal cord from compression when it was exerted by the extravasation of blood.

The diagnostic symptoms of meningeal hemorrhage as given by Gowers and others, and according to my own experience, are as follows: (1) sudden and violent pain in the back, less or more diffused; (2) pain along the course of the nerves passing through the membrane near the extravasation; (3) abnormal sensations,—tingling, etc., and hyperæsthesia, referred to the same parts; (4) spasm involving vertebral and other muscles supplied by affected nerves and also sometimes the muscles supplied by the cord below the seat of the hemorrhage; (5) sometimes general convulsive movements; (6) sometimes spasmodic retention of the urine; (7) consecutive paralytic symptoms, but not usually complete paralysis.

Some points of differential diagnosis between meningeal hemorrhage and extravasation into the substance of the cord should be borne in mind. Symptoms of irritation, such as pain, hyperæsthesia, paræsthesia, and spasm, in meningeal hemorrhage are usually immediate or very early, and may precede paralysis, which is commonly not complete. In hemorrhage into the substance of the cord paralysis may be very complete at first, or rapidly become so, and symptoms of irritation may be very largely wanting. Hemorrhage may, and not infrequently does, involve not only the membranes but also the substances of the cord, giving complex symptoms.

In some cases of intraspinal hemorrhage, whether primary or secondary, trephining might be performed in two places in order to make sure of a good result,—namely, at the supposed seat of the extravasation, and at the lowest portion of the lumbar spine, where, as already indicated, much of the extravasated blood would gravitate. Kronlein, it will be remembered, has recommended that in cases of cranial meningeal hemorrhage, two trephinings should be performed if one is not successful, and several cases of failure in trephining for intracranial hemorrhage have been reported, which would have been successful had the second operation been performed. In like manner, two trephinings of the spine, or a single trephining at the lowest possible place in the lumbar region, might prove successful where failure would otherwise result.

The somewhat frequent occurrence of spinal hemorrhage, with fracture of either the body, spines, or laminæ of the vertebræ, must not be overlooked. The diagnosis of the exact height of the lesion and the decision as to trephining may often have to be settled negatively or positively through a consideration of the question of the probability of a double lesion in cases of accident,—namely, both fracture and hemorrhage. The fracture and the hemorrhage may be situated at different levels, or if the hemorrhage occurs primarily at the seat of the fracture it may find its way down the canal.

I might give many illustrations gathered from medical and surgical literature of intraspinal hemorrhage in which operation would probably have proved successful, but I will confine myself to two, one twenty years old and the other a recent case.

Jackson† reported an interesting case of localized spinal apoplexy, which with our present lights might perhaps have been relieved and a life saved by careful trephining. The patient was a bright girl, 14 years old. While dressing her fingers felt weak. The next day she had a similar weak feeling in her hands. One day later she was unable to move her arms except at the wrists. Later, the intercostal muscles did not act quite freely, and she seemed to lie heavier in the bed. Moist crepitant râles, with a little cough, developed. On the fifth day after the first symptoms, careful examination clearly demonstrated great loss of power in all the voluntary muscles of respiration and in those muscles of the arm, back, and chest supplied by the cervical nerves. The diaphragm became fixed, and there was slight lividity about the cheeks, with a fall in the natural temperature. Sir W. Jenner, who was called in consultation, diagnosticated a clot in the cervical portion of the spinal cord. Death soon

* Lidell, in the "Intern. Encycl. Surg.," vol. iv. p. 379.

† *Lancet*, July 3, 1869.

took place without pain. The whole cervical portion of the spine, but particularly in front and to the left, was embedded in an oblong clot of dark venous blood outside the membranes. The clot ceased at the seventh cervical vertebra.

In this case, as the reporter remarked, the effusion probably took place very gradually, had room to extend itself, and coagulated very slowly and imperfectly. Until the phrenic nerves were interfered with nearly every symptom might have been attributed to hysteria.

The following case was reported by Dr. Arlidge to the Staffordshire branch of the British Medical Association.* The patient, 44 years old, a drinking man, was admitted to the infirmary with complete motor paralysis of the legs, and some weakness in the arms, where, too, formication and numbness were experienced. Sensation was normal, and the legs were painless and their reflexes abolished. His only complaint was of dorsal pain. The bladder was distended, and prior to admission had been emptied by the catheter, but there was no dribbling of urine. Some rhonchi were noted in the upper part of the left lung; heart healthy; bowels confined for four days; the cerebrum undisturbed. The first temperature taken was 103.6°, but the next day it fluctuated between 105° and 106°, and rose to 107.4° the day preceding his death, on September 26, and was accompanied by some delirium. A post-mortem examination revealed a copious hemorrhage within the meninges of the spinal cord, extending downward from the last cervical vertebra for about six inches. The blood was coagulated. No ruptured vessels were found in the cord itself, and no cerebral disease, although the dura mater was very adherent to the skull. There was no fracture of the spine, and no caries of any vertebra.

CARIES AND ANGULAR CURVATURE.

The question of operation for the relief of angular curvature of the spine is one of considerable importance and not to be dismissed hastily. Such an operation has been, until lately, commonly regarded as useless or worse. Its dangers have been overrated, although they are doubtless greater somewhat than most cranial operations. Macewen has reported an extraordinary series of six cases, in which elevation of the posterior laminæ of the vertebræ was performed,—in four with recovery. In favor of operation are,—(1) the fact that by thus allowing the cord to expand backward a present serious paralysis may sometimes be relieved without imperilling the ordinary chances of recovery by anchylosis; (2) when an approximate cure has been effected by anchylosis, but a paralysis or contractures remain, the latter may sometimes be relieved and improved; (3) secondary degeneration of the cord may sometimes be prevented or arrested; (4) when the paraplegia is produced by the connective tissue or other tumors which sometimes accompany angular curvature, rather than by the latter, such growths can be removed. Against operation are,—(1) that cases can sometimes be cured without such heroic procedure; (2) that caries often involve the body of so many vertebræ that operation is often useless or worse.

Trephining in such cases has been performed twice at the Philadelphia Hospital by Dr. White in cases of Dr. Dercum.

A case is reported by Mr. G. A. Wright,† of a boy, 7 years old, who was operated upon January 20, 1888. Disease of the spine was observed one month before operation, with marked angular curvature in the mid-dorsal region, with some lateral thickening. Paresis of the lower limbs was present, and the patient grew gradually worse. Before the operation he had complete paralysis of both lower extremities, with incontinence of urine and fæces. Anæsthesia to the level of the eleventh rib on the right side, and on the left decrease and finally loss of sensation, developed, the anæsthesia extending from the toes to the level of the pubes; from thence to the line of the eleventh rib a condition of analgesia existed. The muscles of the lower limb were in a state of tonic contraction, with marked flexion. The plantar reflexes were exaggerated; cremasteric very feeble, and deep reflexes could not be obtained. His feet were slightly swollen and cyanotic. Until late the symptoms were more marked on the right side than on the left. External and internal treatment were used without avail. Three laminæ were removed in the dorsal region. The theca of the spinal cord exposed was surrounded by a buff-colored, tough, leathery substance, which was cut away with the scissors. The cord did not appear to pulsate, but no point of constriction could be found. Some

* *Med. and Surg. Rep.*, March 23, 1889, vol. lx. No. 12, p. 370.

† *Lancet*, July 14, 1888.

improvement took place in voluntary motion and sensation during a month following the operation. Later, the area of anæsthesia increased, and the condition of the patient became about the same as it was before operation.

SPINAL ABSCESS.

Spinal abscess, like brain abscess, is usually a complication found in connection with disease of the bodies of the vertebræ, and localization rules will not commonly be of much value in making a diagnosis. Pus at any level within the spinal canal will, like blood, usually find its way to the lumbar region. In rare cases suppuration is confined to the space between the dura mater and bone, and when our diagnostic acumen becomes greater we will doubtless in some of these cases be able to trephine with advantage to our patient. Pachymeningitis, with or without suppuration, is generally due to traumatism or vertebral caries, but occasionally cases appear to arise as primary or idiopathic.

In this so-called idiopathic suppuration in the spinal dura mater, trephining low down to give exit to the pus might in rare cases prove a useful procedure. Such a case is that reported by Dr. Robert Maguire.* Other cases have been reported by Mueller, Spencer,† and others.

In Spencer's case of idiopathic inflammation of the spinal dura mater, the patient's illness began with pain in the feet and knees and in the small of the back, frequently shooting down the back to the legs. He had fever and diarrhœa. There was great tenderness on pressure over both lumbar regions. The surface of the body was covered with copious acid sweats. Temperature elevated; pulse rapid, wiry, regular; tongue dry, white fur in the centre, red at the tip and edges. Breathing became rapid and somewhat painful, with a dry, hacking cough. The patient became delirious and his pain in the back very severe, but no paralysis until late. The neck was very stiff. There was a peculiar purplish mottling on the chest and abdomen. He became steadily weaker and died. The whole spinal canal was filled, from within two or three inches of the head down to the sacrum, with thick, creamy pus of a light yellow color and containing numerous shreds of lymph. The pus was found to lie outside the dura mater, between it and the periosteum. The pus had found its way out of the canal through the intervertebral foramina, and surrounded the spinal nerves for some distance. It had also passed out through the laminæ and infiltrated the connective tissue between the muscular structures on each side of the spine, forming collections of various sizes, the largest opposite the lumbar vertebræ. These were not abscesses, but infiltrations of pus into the interstices between the muscles. The tissues around these collections of pus were softened and the spinal nerves were laid bare. The most noteworthy of these collections was one opposite, and to the right of, the sixth and seventh dorsal vertebræ; the pus had here found its way under the costal pleura and projected into the thorax as a tumor, the size of a large hen's egg, lying close to the spine. In this case trephining low down as well as operations in the external abscesses might have been performed.

NEURALGIA.

In rare cases of intractable neuralgia trephining and section of the nerves close to the spinal cord may be tried as a last resort. This operation has been performed in one case by Dr. Abbe,‡ of New York, on the recommendation of Dr. C. L. Dana. The patient, a man 44 years old, was operated on December 31, 1888, and January 2, 1889. His symptoms began with throbbing pain on the posterior surface of the right forearm above the middle; later, paroxysmal pain, giving a peculiar twitching sensation in the thumb, index, and middle finger; after some months there was complete disablement of the hand, which he kept in a stiff position with the fingers semiflexed. The forearm and hand were slightly emaciated, with atrophy of the muscles in the interosseous spaces. The posterior, interosseous, and ulnar nerves were stretched without any improvement. The arm was amputated about the humeral insertion of the deltoid, but without abatement of pain. The morphine habit had been contracted. He had twitchings and tonic contractions of the muscles of the trunk; also the Brauch-Romberg symptoms; he swayed in standing and had a tendency to fall to the right. Knee-jerks were exaggerated, and there was ankle clonus in the right leg. The right shoulder showed muscular atrophy, and stiffness in the neck muscles. A small tender neuroma of the muscular nerve was removed

* *Lancet*, July 7, 1888.
† Ibid., June 14, 1879.

‡ *N. Y. Med. Rec.*, February 9, 1889.

without relief. The right half of the laminæ of the third cervical to the first dorsal spine was removed. Both motor and sensory roots of the sixth and seventh cervical nerves were cut square across outside of the dura. The posterior roots of the seventh and eighth cervical nerves were cut off close to the posterior columns. The patient made great improvement.

In the second part of this paper I will treat of the rules and principles for localizing spinal lesions for operative or other purposes.

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*REMARKS ON THE UNPLEASANT EFFECTS OF SULPHONAL.**

By J. P. Crozer Griffith, M.D.†

IT is the almost invariable history of every newly-introduced drug of real therapeutic value that, at first received with almost unanimous approbation and enthusiasm, the disadvantages which it possesses have gradually come to the fore; so that many physicians whose experience with it has chanced to be unfortunate have gone to the other extreme, and have discarded a remedy really capable of rendering great service. One has but to recall the history of cocaine to become convinced of the truth of this statement. Antypyrin, too, was and, indeed, still is used with a freedom and carelessness which the experience of many has shown to be dangerous; and the once "harmless" acetanilid now produces collapse and death. Yet these and many other of our materia medica, though capable of doing harm, are much oftener productive of the greatest good if properly used, and if their possible dangers are kept in view. In like manner sulphonal came to us highly recommended by its earlier users, and with scarcely a dissenting voice concerning its admirable hypnotic action and its entire freedom from unpleasant effects. Having reported a year ago, in conjunction with Dr. Elwood R. Kirby, the result of some clinical experiments with urethan and amylene hydate as hypnotic agents, I began with great interest and great expectations the administration of sulphonal as a drug of the same class. Experience in a large number of cases has convinced me, however, that though a very valuable agent, it is by no means without the power of producing undesired and quite unpleasant secondary effects; and cases now not infrequently reported in the journals confirm this opinion.

It is not at all the object of these remarks to decry a useful medicine, but to aid in establishing its true value in comparison with other hypnotics; and, by pointing out some of its possible disadvantages, to warn against the unpleasant surprises and disappointments which might otherwise lead to its abandonment. The cases to be detailed by way of illustration have occurred entirely in my own experience. Those quoted from medical literature do not at all complete the list of unfavorable cases reported in the journals.

1. First to be noticed among the disadvantages of sulphonal, and undoubtedly a very prominent one, is its *slowness of action in producing sleep*. This is to be carefully borne in mind in determining the hour of its administration; and the patient should perhaps be notified of its peculiarity, or disappointment and dissatisfaction may arise. This slowness of action has so usually been noticed in the cases under my care, that to cite instances of it would be only tedious. So generally, indeed, has the experience of clinicians agreed as to this point, that Kast, who first employed the drug, has written a second article (*Therap. Monatsh.*, July, 1888), which might almost be called a *defence* of sulphonal, and in which he discusses the reasons of this long delay in action. As a result of experiments on animals and with digestive solutions outside of the body, he advises that it be administered in a finely-pulverized condition, in at least 200 c.c. of a warm fluid, with the evening meal between seven and eight o'clock. The presence of a large quantity of fluid, of hydrochloric acid, peptones, and of salts favors the rapid absorption of the medicine.

2. Another and even greater difficulty to be overcome in the use of sulphonal is the *marked tendency which its hypnotic action has to persist during the succeeding day*. The following case is an excellent example of this:

Case I.—Mr. Robert S. Ulcerative endocarditis with aortic insufficiency and very troublesome insomnia. 20 grains of sulphonal given at about 10 P.M. He did not fall asleep for some hours, but throughout the whole of the next day until late in the afternoon he was so overcome by sleep and so stupid that replies could scarcely be obtained to questions addressed him, and the family became quite alarmed. The same condition was seen on another occasion after a dose of the same

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* Read before the College of Physicians, Philadelphia, May 1, 1889.

† Instructor in Clinical Medicine in the University of Pennsylvania; Physician to St. Agnes Hospital.

size. In fact, sulphonal seemed always in this case to produce sleep lasting only a few hours at night, while upon the next day the patient could scarcely rouse himself. In such a case as this it is not pleasant on entering the room to be greeted by the friends with the words, "Oh, doctor! Those powders, those powders!"

The following less striking case illustrates the same continuance of action after a still smaller dose:

CASE II.—William S. Phthisis and chronic rheumatism. Insomnia usual, though not as a result of cough or pain. After 15 grains of sulphonal, given at 8.30 P.M., the patient slept well, but continued drowsy until noon of the following day.

The same condition is exemplified by some of the other cases to be presently related. (See Cases III., V., VI., XV., and especially XII.)

This prolonged hypnotic action has frequently been referred to by writers. L. L. Johnson (*Med. News*, September 8, 1888), for example, reports a case in which the patient promptly fell asleep after 30 grains of sulphonal given in the evening, but was very somnolent until afternoon of the next day. In smaller amount the drug acted very well. Algeri (*La Riforma Medica*, No. 233, 1888) says that the action of large doses continues through the following day, and Oestreicher (*Berlin. klin. Wochenschr.*, June 18, 1888) has seen sleep remain absent during the night, but persist through the following morning. Lovegrove (*Brit. Med. Jour.*, May 26, 1888), too, states that the effects on patients to whom he gave the drug were very discouraging. For several hours after taking it no appreciable action could be noticed, but during a great part of the next day there was extreme drowsiness and considerable cyanosis. The case of Schotten's, presently to be quoted, is of especial interest in this connection. Kast discusses this "postponed action" in the article referred to, admitting that it is often witnessed, and that it constitutes a great disadvantage when it is necessary that the patient be actively employed during the day. For this difficulty he has no special remedy to propose in addition to the method of administration described, except that the dose be carefully adapted to the individual.

3. But this *determination of the individual dose*, in order to overcome the difficulties mentioned, itself constitutes a sometimes insurmountable obstacle to the use of sulphonal. I have frequently found that the amount necessary to produce satisfactory sleep was followed by the prolonged effect on the following day, or by various unpleasant after-effects presently to be described, while any smaller amount failed to bring the desired result during the night. In quite a number of cases it was observed that a small dose merely served to render the patient uncomfortable in the night and upon the following day, while a full dose procured satisfying sleep, and the morning was free of prolonged action or disagreeable after-effects. It would seem sometimes as though sound sleep were necessary to exhaust, so to speak, the action of the medicine. Matthes (*Centralbl. f. klin. Med.*, No. 40, 1888; *Wien. med. Blätter*, December 13, 1888), too, reports a case in which very marked disagreeable after-effects appeared to develop because the sulphonal-sleep had been much disturbed by outside causes.

The following cases are examples of the fact that a large dose is sometimes less objectionable than a small one, and are also illustrative of the difficulty in determining the proper amount to give:

CASE III.—George H., aged 15. Rheumatoid arthritis. Troubled with nervous insomnia. 15 grains of sulphonal were almost entirely without hypnotic action, and 25 grains had but little more effect. After these smaller amounts there were short, bewildered, troubled periods of sleep throughout the night, with a tendency to talk at random; and upon the days following the patient was drowsy and uncomfortable. 30 grains, however, produced six hours' continuous, restful sleep, entirely without unpleasant after-effects.

CASE IV.—Miss O. Hysteria; great nervousness and insomnia. Sulphonal in a dose of 30 grains, given on three successive evenings, was only productive of short, delirious dozes, and upon the morning following there were nausea, headache, and generally unpleasant sensations; so that the patient begged that no more of the drug be given her. A further trial was made, nevertheless, and 40 grains were administered. This gave her six hours' sound, uninterrupted sleep, with no unpleasant sensations on the next day. The 40-grain doses were continued during some weeks with great satisfaction to the patient. She later found that 20 grains were often sufficient, but if very nervous she was obliged to take the larger amount.

The dose probably most often recommended by writers is 1 to 3 grammes, and it has been repeatedly claimed that unpleasant effects only follow the administration

of a dose unnecessarily large. The cases which I have described show that this is not the fact. I should say, too, that I have repeatedly, and indeed usually, commenced with a small dose, but have generally found it insufficient: and can recall but one instance in which 10 grains exerted a hypnotic action. Rosin (*Berl. klin. Wochenschr.*, June 18, 1888), who has carefully tested the size of the necessary dose, found that 1 gramme was seldom, if ever, enough to cause sleep, except in those cases in which starch, given under the name of sulphonal, had an equally good effect; and Funajoli and Raimondi (*Archiv. italiano per le mal nervos.*, etc., 1888, xxv. 325) believe that the efficient dose for men is 4 grammes and for women 2 grammes, and that the best results are not obtained until the third night of administration. Too much attention cannot be given to the proper method of testing hypnotics, as so well described by Rosenbach (*Berlin. klin. Wochenschr.*, June 11, 1888), and to the absolute necessity of eliminating the influence of psychic impressions before concluding that sleep is due to the medicine given. The relief of pain and the production of sleep by morphine powders composed of sugar of milk is a ruse too well known to need remark. Matthes (*loc. cit.*) comments on the difficulty in determining the dose of sulphonal, since it not only seems to vary greatly with different persons, but at different times with the same individual. He gave 5 grammes to one patient without any effect whatever, while in another case which he reports the drug had been used repeatedly with good results; but on one occasion a dose of only .5 gramme occasioned very unpleasant symptoms.

A somewhat similar instance is the following:

Case IV.—Lizzie H. Insomnia. 40 grains of sulphonal were taken on retiring on four consecutive nights. On the first three a rather restless slumber, in which the patient talked somewhat, began in one to one and a half hours; followed at about 3 to 4 A.M. by a deep sleep lasting until 8 or 9 in the morning, when she woke refreshed. On the fourth night, instead of passing into the deep, quiet sleep at 3 o'clock, she rose, still unconscious, walked about the room with a candle in her hand,—a rather dangerous proceeding,—and screamed at the top of her voice. As she had never been known to act in this way before, her relatives were not without reason in considering the medicine at fault, and in urging her to take no more of it.

4. *Unpleasant secondary effects* have been already alluded to in these notes as constituting one of the disadvantages of sulphonal, and some of them have been mentioned in the cases already described. Many writers have observed them, so that the claim made by the makers of the drug and by some of its users, that it is totally without disagreeable secondary action, is not substantiated. Zerner (*Wien. med. Wochenschr.*, November 10, 1888), indeed, estimates that these effects are seen in 10 per cent. to 12 per cent. of all cases, and Matthes (*loc. cit.*) says that they were noted in a majority of his patients. Prominent among them may be mentioned a *condition of excitement* developing instead of the wished-for sleep. The following case illustrates this condition:

Case VI.—Mrs. McC. Disseminated sclerosis; extreme and persistent insomnia, for which various hypnotics had been used in vain. Small amounts of sulphonal had no effect whatever. Finally 60 grains were given on each of four consecutive nights, but the patient appeared only to become more excited and nervous, and more wakeful, if possible. On the days following the nights of administration she was drowsy and uncomfortable.

Other illustrative cases are the following:

Case VII.—Jennie S. Neurasthenia; anæmia. Annoying insomnia was relieved nicely by paraldehyde, but 30 grains of sulphonal were absolutely without action. 40 grains given early on two successive nights made her very excited and semi-delirious, without producing any desire for sleep until about 4 A.M. From this time until the evenings of the days following she experienced nausea, headache, and vertigo, and was so generally uncomfortable that she flatly refused to take any more of the drug.

Case VIII.—Mrs. E. Rheumatoid arthritis; simple insomnia. 35 grains of sulphonal had a very exciting effect throughout the whole night, producing short, dreamy snatches of sleep. On the next day there was nausea and drowsiness.

Case IX.—Mrs. B. Simple insomnia. 20 grains of sulphonal only excited the patient through the whole night, and produced no sleep whatever.

Case X.—Tak Sing. Obstruction of the superior vena cava; persistent insomnia. 15 grains of the drug were given at 10.30 P.M., and repeated in an hour. The patient slept about half an hour after each dose, but for the remainder of the night was unusually restless.

CASE XI.—James B. Alcoholic delirium of a mild type. The patient was restless, never violent, often wanting to get out of bed, but easily restrained. He was sleeping quietly at 4 A.M., when 40 grains of sulphonal were given as a test, and in about half an hour he became very unruly and delirious. He had not acted so badly before, nor afterwards until the like amount was administered on two other occasions, when the same results were observed. On another day 20 grains, given at 7.30 P.M., produced so much restlessness and wakefulness that 45 minims of amylene hydrate had to be administered at midnight, after which he went to sleep.

It is worthy of note in this case that paraldehyde, chloral, or the bromides also exerted a quieting influence; while hyoscine produced on two occasions effects similar to those of sulphonal.

One more illustration may be given.

CASE XII.—Mr. H. Mitral regurgitation. Troubled greatly with insomnia. 40 grains of sulphonal administered on each of several nights produced for some hours intense drowsiness and the desire to go to sleep, without the power to do so. The patient walked the floor, and felt "trembling" and nervous, and queried afterwards whether the powders could have made him so "lightheaded." After about two hours of this condition he fell asleep, and was very drowsy during all of the day following the administration of the drug. His wife, too, remarked that "the medicine had an effect on his brain."

A tendency to an excited mental condition *accompanying* but not *replacing* sleep is shown by Cases III. and IV., but especially by X.

Temporary *delusions* after the ingestion of the drug are reported by Fürbringer (quoted by Zerner, *loc. cit.*), and may be classified in the same category with the excited mental conditions I have reported.

Nausea, *dizziness*, and *headache* have been already mentioned in Cases IV., VI., and VII. as effects which may follow the ingestion of sulphonal. A similar instance is as follows:

CASE XIII.—Sallie C. Mitral stenosis. 30 grains of sulphonal had no effect for several hours. On the following day the patient felt uncomfortable, and suffered from headache and dizziness. 40 grains given on another occasion produced five to six hours' sleep, but the after-effects were the same.

Matthes (*loc. cit.*) says that in some of his cases *ringing in the ears*, headache, and dizziness were experienced on the next day; and that in two instances *vomiting* occurred. Among the patients of G. W. Rachel (*N. Y. med. Presse*, November, 1888) was one in which 1.5 grammes of sulphonal were followed by fourteen hours' sleep, and later by a sense of dizziness. Otto-Dalldorf (*Deut. med. Wochenschr.*, August 23, 1888) and Rosin (*loc. cit.*) have likewise witnessed dizziness, which sometimes lasted nearly the whole of the next day. Rabbas (*Berlin. klin. Wochenschr.*, April 23, 1888) has seen vomiting on the morning after the ingestion of sulphonal, and in another patient *diarrhœa* occurred; but he is not certain that these symptoms were attributable to the action of the medicament. *Heaviness in the head* and dizziness were observed by Sachs (*N. Y. Med. Rec.*, October 6, 1888) in a patient who had taken two doses each of 2 grammes three hours apart. Schwalbe (*Deut. med. Wochenschr.*, June 21, 1888) has in several instances observed dizziness, headache, nausea, and in one case vomiting, after a dose of only 1 to 1.5 grammes, and S. G. Burnett (*N. Y. Med. Journ.*, March 2, 1889) has seen two instances of vomiting.

The production of a marked *sense of fatigue*, of *depression*, and of *confusion of mind* is not infrequently referred to by authors. J. Fränkel (*Berlin. klin. Wochenschr.*, July 23, 1888) has found these symptoms so decided in a number of patients that the drug could no longer be given them; Matthes (*loc. cit.*) refers to the sense of exhaustion and depression as the secondary effects most frequently observed, and Oestreicher (*loc. cit.*) has seen them in one or two out of eighteen cases. In one of these it was very pronounced, and the patient refused to take the medicine again. Ott (*Prag. med. Wochenschr.*, October 3, 1888) refers to the sense of fatigue on waking as the chief bad symptom which the drug leaves behind it. He reports a case in which 3 grammes caused vomiting and an attack of *fainting*. The confusion of mind is exemplified in several of the cases I have reported, especially in Case XII.

In two instances I have seen full doses of sulphonal produce a *disturbance of gait*.

CASE XIV.—James M. Aortic regurgitation; insomnia. 40 grains of sulphonal were followed by sleep lasting most of the night; but on the next day the patient suffered from swimming in the head, and oscillated while walking.

CASE XV.—Mr. C. Plethora. Greatly troubled by insomnia. After 40 grains of

the drug, given in the evening, the patient slept well, but could with difficulty rouse himself on the following morning, and felt all day as though stumbling over something.

The occurrence of a drunken, staggering gait has also been reported by Zerner (*loc. cit.*) and by Otto-Dalldorf (*loc. cit.*). Bornemann (*Deut. med. Zeitung*, November 26, 1888) reports a case of the morphia habit in which 60 grains of sulphonal were taken at about 9 P.M., and one-half of this amount at 1 A.M. As a result there developed very marked muscular inco-ordination with great mental depression, lasting to some extent during six days. This is a case in proof of the observation already made by Ziehen (*Arch. f. Psychiat. u. Nervenk.*, B. xx. H. 2), that severe motor inco-ordination and a somnolent condition are liable to appear if sulphonal be given at the same time with opium or to opium habitués. Still another case in point is that recently published by Fischer (*Neurolog. Centralbl.*, 1889, No. 7), in which the drug, given repeatedly to a morphia habitué, produced on two occasions not only ataxia of the limbs, but indistinctness of speech. Burnett (*loc. cit.*) mentions the occurrence of muscular inco-ordination after the ingestion of sulphonal. He further reports *intense thirst* and great *dryness of the mouth* produced by it in three cases.

Engelmann (*Münch. med. Wochenschr.*, October 16, 1888) details a case in which 3 grammes produced an erythematous *eruption* over the breasts and inner surface of the arms, attended by considerable itching. Another instance of an eruption occurred in a case of Schotten's (*Therap. Monatsh.*, December, 1888), in which 2 grammes of sulphonal, given on each of two succeeding nights, produced but little sleep at night, but depression on the following days. After 3 grammes on the third evening the patient slept all night, but seemed completely exhausted and extremely drowsy on the next morning, and suffered from persistent headache and loss of appetite. This condition lasted four days, when improvement set in, and simultaneously an eruption exactly resembling measles appeared over the whole body. Kisch (*Berlin. klin. Wochenschr.*, No. 7, 1889) reports three interesting cases of ill effects from sulphonal. In the first, a patient with hemiplegia from a recent apoplexy slept the entire night after taking 1 gramme of the drug. On the next morning, however, he had complete *aphasia*, and only after eight to ten hours did this slowly disappear, the patient meanwhile feeling very weak. The second patient, after taking 3 grammes of sulphonal in divided doses during the night, felt wretched and exceedingly languid on the following morning, and complained of great depression and of a feeling as though his senses were leaving him. The *pulse was also retarded*, beating only 38 in the minute, and this symptom did not disappear for several hours, and under the use of stimulants. The third patient, a man 62 years old, slept soundly after 1 gramme of the drug. On the following morning he was horrified to find that he had had a *nocturnal seminal emission*,—the first for over ten years. He also felt as though stupefied during the whole day, and could not leave his bed.

Schmey (*Therap. Monatsh.*, July, 1888) details most unpleasant results following the administration of sulphonal to a case of angina pectoris from arterio-sclerosis. Nitrite of amyl had reduced the attacks to one to two a day, and amylene hydrate produced sleep very satisfactorily. As the patient complained of the taste of the latter, the author administered 2 grammes of sulphonal, soon after which *attacks of angina* came on with great violence and with only a few seconds' intermission during the entire night, without any real sleep. For the next two days the attacks were unusually frequent and severe. It is true that the occurrence of these symptoms may have been only a coincidence, and Müller (*Therap. Monatsh.*, August, 1888) claims that this is proved by the fact that in a case of arterio-sclerosis in which he used sulphonal no unfavorable action was observed. This is, of course, only negative proof. Burnett (*loc. cit.*) reports a case of arterio-sclerosis in which the patient became cyanosed and semi-comatose for twelve hours after taking it. He believes it to be contra-indicated in this condition. Lovegrove (*loc. cit.*) has also witnessed cyanosis produced by sulphonal, as has been already stated.

5. Finally, the *uncertainty of hypnotic action* is one of sulphonal's disadvantages,—a fault which it shares more or less with all hypnotics. Cases of failure of action on the part of sulphonal are too numerous in the literature to allow of quoting from them. Among those who have been unfavorably impressed by the drug on the ground of its inefficiency may be mentioned Wilson and Hutchinson (*Med. and Surg. Reporter*, June 9, 1888), Lovegrove (*loc. cit.*), Salgo (*Wien. med. Wochenschr.*, No. 22, 1888), and Regis (*Journ. de Méd. de Bordeaux*, July 1, 1888). Several of the cases I have reported might serve as examples, but

the following are perhaps still better illustrations of its occasional lack of hypnotic power, though given in full or even very large amount.

CASE XVI.—Mr. W. Fracture of spine. No pain, but persistent insomnia. 30 grains of sulphonal in the evening had no effect whatever.

CASE XVII.—Mrs. F. Paraplegia; persistent insomnia, and some pain. 30 grains at bedtime did not produce sleep.

CASE XVIII.—Mr. B. Cancer of face; insomnia independent of pain. Opium often used in full but not excessive doses. 40 grains of sulphonal at bedtime only made him drowsy and uncomfortable. On the next night the same dose was given and repeated at midnight. Even this large amount produced but little sleep, and the patient complained on the following day of dizziness and headache, and of feeling "queer." The same absence of beneficial action was observed after 80 grains given on another occasion.

The large majority of writers, nevertheless, pronounce favorably for sulphonal. My own experience with it has been for the most part satisfactory; for the number of cases in which no unpleasant effects have followed or supplanted the refreshing sleep obtained by its use much exceeds the number from which I have reported examples here. The instances detailed, with those reported from the experience of others prove, however, that it is to be used with a proper appreciation of the bad results which may follow, that the time of administration is to be carefully determined, and that the dose should be accurately adapted to each individual case.

The chief disadvantages of sulphonal may, then, be recapitulated as follows: 1. Its hypnotic action usually develops very slowly. 2. This action is very liable to be prolonged throughout a greater or lesser part of the following day. 3. It is difficult to determine the dose which may be given with effect and with comfort in each individual case, and this dose may vary at different times in the same case. 4. The drug is liable to produce unpleasant secondary effects, which may even replace the primary hypnotic action. Chief among these are mental excitement, nausea, vomiting, dizziness, headache, languor, exhaustion, depression, and a staggering gait. These symptoms may appear either after large or after quite small doses. 5. It very often fails to exert any hypnotic action, either in any dose whatever, or in any amount which can be given with comfort to the patient.

Though I have expressed myself as in general favorably impressed with sulphonal, yet I confess to a decided preference for paraldehyde, and especially for amylene hydrate. With the latter drug my experience has been especially satisfactory, and disagreeable results following its administration are much less frequently reported in the journals than in the case of sulphonal. The objection that its taste is disagreeable is entirely removed by giving the full dose (45 minims) in 3 capsules of 15 minims each. It is especially to be preferred when a rapid hypnotic action is desired, or where there is not time to learn by repeated trial the amount of sulphonal suited to the patient in question. In cases, however, in which the stomach is irritable, it is probable that the latter will usually be more easily tolerated.

BRAIN WOUNDS, WITH EXCESSIVE LOSS OF STRUCTURE.

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(Concluded from page 237.)

THE two succeeding cases occurred in the practice of my father, Dr. C. M. Stockwell, and his associate and brother-in-law, Dr. Geo. B. Willson, and were likewise matters of personal observation to the writer, at the time a medical student. They are reproduced in condensed form from memoranda made by Dr. Willson, which, with the breech-pin in question, fell into my possession upon his demise.

2. June 4, 1869, Drs. Stockwell and Willson were summoned to S. W. B., a cabinet-maker, aged 22, wounded by a circular table-saw sixteen inches in diameter, with one-fourth inch "sett" of teeth. He was found at the shop where the accident occurred, seated upon a tool-chest, unsupported and unaided. Not more than fifteen minutes elapsed between the moment of injury and the arrival of the physicians. There had been considerable hemorrhage, judging by the clots upon his neck and stains upon clothing and floor, but this had been controlled by a workman's apron wrapped tightly about the head. He denied any sense of suffering or pain, save while coughing, which occurred in frequent, persistent, hacking paroxysms, first beginning when he assumed an upright posture after the accident. He volunteered the information that the injury was received while in the act of withdrawing himself from beneath the saw-table, where he had been engaged in adjust-

ing and tightening the belt connecting the pulley of the saw journal with another on a shaft beneath the floor; and thus, in some unaccountable manner, his head had been brought in contact with the revolving disk, that at the time was moving with a velocity estimated at from three thousand six hundred to four thousand revolutions per minute.

On removing the apron from his head, a wound seven inches in length, more than three inches in depth, and corresponding to the *sett* of the saw, was revealed, extending across the sagittal suture, dividing the longitudinal sinus, falx, and branches of the middle meningeal artery. Beginning at the superior temporal ridge of the left parietal bone, it reached diagonally across the head to a point on a level with the tip of the right ear. Coughing with each inspiration caused the cerebrum to shrink from the skull, admitting air beneath the dura, that, with each expulsive effort mingled with blood and produced a frothy fluid, which at times was hurled to a distance of more than a yard.

While cleansing the wound exteriorly, the cough suddenly ceased, but whether spontaneously, or as the sequel to a quarter of a grain of *morphinæ acetate*, administered dry upon the tongue a few moments before, is a matter of question and doubt. With a grooved director, the interior of the wound—the cerebral walls and bottom of the cavity—was carefully scraped, bringing away considerable quantities of bone-dust and crumbs, when the cut was packed with a well-oiled piece of chamois skin quadruply folded, and then closed, save at its most dependent extremity, with adhesive strips. Next the patient was conveyed in a carriage to his dwelling, five blocks away, and relegated to a bed in a cool room. . . .

Further details of the case are of little moment save to add, the head was dressed every three or four days, or as occasion demanded, during a period of six weeks, after which the patient resumed his former occupation. He at no time suffered from pain, and, aside from the morphine administered immediately after the accident, received no medication, except on one occasion *half* of a seidlitz powder. Shock was manifestly considerably less than would accrue to breach of muscular continuity of equal extent; and this will be observed to hold true of all open and sharply-incised wounds of the cerebrum. Two years subsequent to the accident, the man removed to central Ontario, and all trace of him was lost, though I was recently informed he yet lived, in full tide of health and possession of intellectual faculties.

In the *North American Medico-Chirurgical Review* for January, 1858, an almost parallel case is reported by Dr. Ellerslie Wallace, of Philadelphia. The wound, inflicted by a saw similar to that in the case above noted, was four and a half inches in length by one-fourth in width, and extended horizontally across the skull along the coronal suture, lacerating the brain and dividing the longitudinal sinus. The patient, a girl of 10 years of age or thereabouts, recovered without a single untoward symptom.

3. On June 2, 1860, I accompanied my father and Dr. Willson to an adjoining county to see one Wilson Shaw, a farmer's boy, 20 years of age, who was reported to have had "a gun-barrel blown completely through his head." The accident occurred the day before, from careless firing of a converted flint-lock musket of the "Tower pattern," known to have been loaded for at least fourteen months.

The force of the blow felled young Shaw to the ground, but did not render him unconscious, for he immediately arose, and, unaided and alone, walked to the house, thirty rods away.

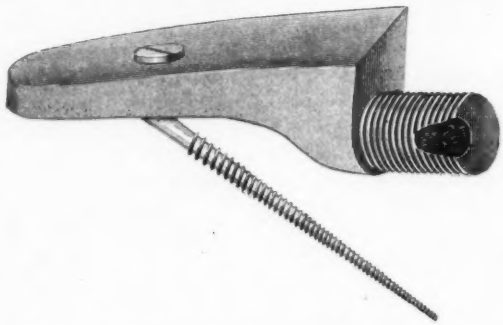
On removing the bandage, the left eye was discovered to be slightly protruding; the surrounding tissues bruised and ecchymosed; and the forehead, generally, blackened with powder. To the left of the median line appeared a large, irregular, and somewhat triangular-shaped opening, a trifle more than two inches long, and about an inch and a quarter in extreme breadth, the upper portion tapering to an obtuse point; the base of the triangle corresponded to the inner half of the superciliary ridge of the same side. From this opening disintegrated cerebral matter was slowly oozing, the man having lost, according to the testimony of the most conservative of the witnesses, "not less than six or eight heaping tablespoonfuls of brains." There was not, neither had there at any time been, loss of consciousness or evidences of paralysis. He made no complaints; his senses, naturally acute, remained unimpaired; the functions were undisturbed; and he denied suffering, except that the head felt "sore" and the left eye "slightly painful." His sole anxiety, apparently, was that a foreign body (which he believed to be a portion of the gun-barrel) be removed from its lodgment within the brain at a point he defined as "the middle of the head between the ears."

On pursuing the examination, the tip end of a screw was discovered just within the opening through the skull that extended back into the brain; and an attempt to remove it revealed its connection with some solid body beyond. By the introduction of a finger, a solid iron block, apparently the size of the little finger, was defined lying at right angles to, and penetrated by, the shaft of the screw, forming as it were a T. This second piece further appeared to lie in a slanting direction, upward and backward, the lower portion hooked behind the posterior edge of the roof of the left orbit, and the lesser wing of the sphenoid, in the middle fossa of the base of the skull; its point projected into the sphenoidal fissure, pressing upon the nerves and tissues of the posterior portion of the orbit, procuring protrusion of the eyeball and the pain complained of when traction was made upon the screw. The other, or superior, extremity of the block reached to the vertex, where it was solidly jammed against the under surface of the skull immediately to the left of the *falx*. Under the circumstances, every attempt to lift the lower arm from behind the lesser wing of the sphenoid, by employing the screw as a lever, proved futile.

Dr. Willson endeavored to push the screw upward and backward in order to tip the upper arm of the second body downward towards the occiput, and thus relieve the opposite extremity from its entanglement in the sphenoidal fissure. This also was a failure, however, as the point of the screw could not be pushed backward sufficiently to pass within the skull at the upper edge of the wound; it appeared to be firmly wedged in the solid with which it was connected, at or near its head.

After vain attempts to shorten the screw by means of such tools and appliances as were available, a V-shaped portion of the skull was excised at the upper portion of the wound, which then permitted the screw to be tilted upward and brought within the cranial cavity, thus relieving the second body, which, by being pushed backward, was disengaged, lifted from its resting-place, and removed. It proved to be the *breech-pin* of the gun,—a solid mass of iron of nearly five ounces weight, two and three-fourths inches long, five-eighths broad, and perhaps one-fourth thick along its posterior and flattened portion; the cylindrical or screw end of the breech-pin where it entered the barrel was five-eighths of an inch in diameter and nine-sixteenths long. The screw that passed through the pin was two and seven-eighths inches long, with an average diameter of one-eighth inch; and, at the point where it left the pin, it was bent to a degree that rendered it immovable save for slight rotation.

Pursuing the search in quest of other possible foreign bodies, it became necessary to



remove fully a teacupful of disintegrated brain-substance; and in all not less than six or eight ounces by measure must have been taken away, including that lost the day before, if the estimates of attendants were at all reliable. And, at a depth of four and a half inches (actual measurement), imbedded in the left posterior lobe, were found two pieces of skull a trifle more than half an inch square, besides several smaller fragments and crumbs. During this time several experiments were instituted to determine the sensibility of the brain, which did not seem at all tender; but the patient, though blindfolded, was able to determine the nature of the substance or object introduced, by the sense of feeling, as accurately as he could have done by his finger-tips. A pair of blunt scissors he declared to be "cold and smooth, like polished iron or steel," and that the points rested "the least bit higher, but just midway between," the two ears. Even when the walls of the cavity were scraped to remove the last ves-

tiges of bone and disintegrated brain-substance, he denied any feeling of pain, though he defined the act with accuracy. When surprise was expressed at the definiteness of feeling and perception, Shaw laughed heartily, remarking, "Why I can feel everything in there—just as plainly as if applied to any other portion of my body."

After cleansing the cavity, and edges and surfaces of the membranes, a plug of oiled lint was introduced, and secured by adhesive strips, in turn covered by a bandage. *Hernia cerebri* supervened towards the end of the first week, but this being freely excised, gave outlet to a considerable accumulation of pus, when no further difficulty was experienced; the patient was discharged four weeks later. Abandoning the occupation of farm-hand, he now became for a brief period a school-teacher, and subsequently and successively blossomed out as a phrenological lecturer, apostle of teetotalism, justice of peace, map agent, book canvasser, labor agitator, and saloon-keeper. He recently died in or near Grand Rapids, Michigan, from *mania-à-potu;* and the only mental aberration ever detected was a decided disinclination to live honestly, or to meet his lawful debts and obligations, the bill for medical attendance included.

4. In July, 1872, I was summoned to see a Norwegian laborer, æt. 42, who had received a transverse cut across the head and through the brain, extending from one and one-eighth inches above, and one-fourth inch anterior to, the left ear, to a point precisely opposite the tip of the opposite auricle. The wound was made by a band-saw bolted to the circumference of a horizontally-moving disk, twenty-seven feet in diameter, with a claimed velocity of two thousand revolutions per minute. This saw, employed to cut bolts into staves for wine-casks, had the general appearance, and somewhat of the application, of a trephine. The wound measured nearly eight inches in length, following the contour of the head, seven-eighths in average width, and from one and a half to two inches in depth, the scalp and brain being excessively lacerated at the most dependent extremity of the opening. Aside from what may have been lost at the moment of injury and before my arrival, above two fluidounces of disintegrated and mangled cerebral substance were removed. The case, which in general characteristics was very like Nos. 2 and 3,. progressed favorably to recovery, save once, when, evidences of compression supervening, the brain was freely incised at the lowest portion of the wound, affording exit for a considerable accumulation of purulent fluid. Seven or eight months after the patient was discharged, he died suddenly from the effects of a prolonged and severe alcoholic debauch; and an autopsy failed to reveal any cerebral complication. On the contrary, the brain was completely healed, and every way normal, giving no evidences of injury; and but for the evidence afforded by the cranial vault, it would have been difficult for a stranger to locate the wound.

5. By an explosion in the dust-shaft of a flouring-mill, a splinter of pine, seven-eighths square and seven inches long, was driven into the brain of a lad 9 years old, entering beneath the left zygoma. The case was almost a parallel of Case 1, save in the character of the missile, and the fact that the splinter failed to penetrate the vertex. Removal of the foreign body was accomplished only with considerable difficulty, and followed by a copious discharge of blood and disintegrated cerebral matter. By keeping the wound open five weeks, a good recovery was made, though at one time, owing to neglect on the part of the child's parents, the tract became blocked, and an attack of encephalitis supervened that quickly yielded to treatment, when free exit was provided for accumulated inflammatory products.

6. This case, reported by Dr. Folsom, of California, exhibited a circular-saw wound extending longitudinally across the skull and dividing the parietal bone just to the left of the sagittal suture. It extended from a point on the frontal bone one and a half inches above the nose to a point below, and a little to the left of the occipital protuberance; was nine inches long by one in breadth, and two and a half to three inches deep. Recovery perfect, with no impairment of mental function.

7. A large dog, 4 years of age, was wounded by the premature discharge of a gun, the missile being a "green cartridge" of swan-shot (packed in bone-dust, within a copper-wire cage), that flies as a solid for a distance of about sixty yards ere the pellets are dissipated. It ploughed a groove three fingers deep and two broad in the anterior portion of the skull immediately above the eyes. For the sake of study and experiment, the unfortunate was not, as is usually the custom in such cases, "put out of his misery;" and with prompt and careful attention, wholly recovered, the cerebral portion of the wound healing by

granulation. Unfortunately, I was not able to determine the amount of brain-substance that was lost, though it must have been considerable. Exuberant granular points were touched daily or as occasion demanded, with silver nitrate, or acid chromic, and the entire tract always dressed with surgical oakum, maintained in position by means of a bandage and hood. There was never any manifestation of severe suffering. The accident occurred five years since, and the creature yet lives, apparently in good health, and no way different from other canines of his age and class, and is in considerable demand as a stock dog. I am also informed of a like accident occurring to a valuable brood mare owned by a brother practitioner, but have been unable to secure full particulars. The loss of cerebral substance is reported to have been considerable, however, and vision in the right eye completely obliterated. Since the injury the animal is accredited with better temper and greater tractability, which, perhaps, is less the result of changes within the brain than the lesson inculcated by the accident. She has given birth to four foals, the last in July, 1888; has been worked upon a farm, and at intervals driven both single and double upon the road.

As a matter of contrast, I present two more cases, the first a patient of my own, the other that of a brother practitioner, who kindly placed the same at my disposal.

8. The outlines of this case are almost exactly identical with those of his countryman as detailed in Case 4. The wound was in the same locality, inflicted in the same way (the sequel of careless curiosity), and by the self-same saw. Progress towards convalescence was every way favorable until the third week, when neglect to comply with instructions permitted the closing of the wound externally. I was summoned hastily one morning, only to find him writhing in a convulsion, and with all the symptoms of cerebral compression, which were almost instantaneously relieved by the thrusting of a finger-knife deep into the cerebral substance at the most dependent extremity of the wound, thereby affording speedy exit to a large accumulation of purulent fluid. A fortnight later I was again sent for, only to witness a repetition of the evidences found on the former occasion (I should say, I did not see the patient during the interval, as he went away without my consent or knowledge, and returned only the day before); but the prejudice of friends and countrymen assembled would not permit me to repeat the incision, and life lapsed a few hours later. An autopsy was obtained, however, which revealed two ounces of pus walled up within the cerebral substance.

9. A young man, 22 years of age, was wounded by the accidental discharge of a pocket pistol carrying a .22 bullet. The missile entered half an inch to the right of the median line of the forehead, and one and one-fourth inches above the superciliary ridge. For a time he did well, and resumed his clerical duties the fourth week following the injury, though the wound still continued to discharge pus in moderate quantities. Suddenly *hernia cerebri* appeared, and his medical attendant, following the teachings of his *alma mater*, of text-books, and the advice of counsel, feared to resort to any measures looking to relief except the authorized *repression by pressure*. The cessation of the discharge was coincident with the development of the hernia, and was succeeded by evidences of compression, in turn followed by death. *Because the pus was surrounded by cerebral tissue, the patient must needs be allowed to die in obedience to an absurd fetich!* The autopsy revealed two fluidounces of pus situate less than an inch from the opening of the wound, and consequently easily and safely accessible to the knife. The bullet, two inches farther back, was imbedded and encysted in the right posterior lobe, and its track obliterated, conclusively demonstrating that it was out of the way for harm, and no way immediately responsible for the untoward result.*

All these cases, fatalities as well as recoveries, fully illustrate the necessity in cerebral injuries, *first and foremost*, of providing for *free drainage*. Therein lies the secret of recoveries that, viewed in the light of the hour, were deemed little less than miraculous. In the very magnitude of the wounds and lacerations lay the elements of safety,—first, in the avoidance of concussion; and, second, a minimum of compression, it being local merely, instead of general distribution.

In numerous experiments undertaken upon lower animals, every case recovered promptly and readily when the wound was persistently kept open, permitting free escape of inflammatory products, and antiseptically treated, and with less detriment to general health

* Two other cases that will repay perusal may be found in the *American Journal of the Medical Sciences*, —that of Dr. H. D. Noyes, vol. lxxxiv. p. 45; and another, of Dr. Geo. Burr, same volume, p. 56.

than commonly accrues to wounds and injuries of other portions of the physical economy. Further, these experiments seem to conclusively demonstrate comparative freedom from danger in all brain wounds, within certain limits, where they are accompanied by free openings into the cerebral vault; also that they, as a rule, are *less fatal* and more amenable to surgical measures in proportion to the loss of tissue, there being exceptional freedom from shock, surgical fever, and reflex phenomena, than breaches of muscular continuity, or fractures of long bones, providing the demands of sanitation (antisepsis and drainage) are fairly met. In all the cases that have come under personal observation the hemorrhage has been slight, or at least inconsiderable, as compared with the area of the lesion, and all exhibited large openings through or into the cranial vault and the meninges, with large suppurating surfaces, and great toleration towards the knife and curette.

By teachers and text-books generally, *hernia cerebri* is regarded as a *noli me tangere*. This, however, far from being true, is a mere *fetich*, which requires but little forethought to banish to the realms of superstition, where it rightly belongs. Reflection must convince the most sceptical that the "masterly inactivity" regarding surgical interference is a gross error resultant upon blundering misconception. For instance, Professor Chas. Nancrede, of the Philadelphia Polyclinic, while discussing the problem (?) of cerebral hernia, voices the opinion of the profession at large when he says,* "*The less done the better, as the cause primarily is loss of support.*" And, again, that interference "*must be avoided, especially when granulations are springing upon or around the protuberance, as we thus remove pressure and encourage growth.*" Atop of all this he insists upon resort to compression, with a view to forcing the mass back upon its matrix.

With such teaching as this, the impression must assert itself that those who voice and corroborate the fallacy have never chanced to observe the brain of man or other animal while in the full tide and vigor of life. Nothing is farther from the truth than the supposition the cerebral organ is a soft, pulpy, pudding-like mass, or a gelatinous substance, such as is sometimes found some hours after death. On the contrary, the brain is a firm and elastic organ, possessed of peculiar homogeneity. Its individuality is such that it everywhere leaves its impress upon the inner table of the skull during development, for it, instead of its osseous protection, gives form to the head. Again, the literature and discussions devoted to *hernia cerebri*, curiously enough, deal for the most part with *effects*, to the exclusion of *causes;* and physiology and pathology have been forgotten or ignored in anxiety to avoid responsibility and secure "*safe*" treatment,—all as the sequel of a primitive and barbarous superstition.

Let us look at this matter as it is, and not as superstition and fetichism teaches. *Hernia cerebri* is invariably the sequel of one of three conditions: First, a tumor composed wholly or partially of disorganized brain-substance; second, brain matter protruded by inflammatory processes within the cranial vault or cerebral tissue; third, exuberant or excessive granulations springing from damaged brain-substance.

Now, if the hernia is composed of disorganized tissue, certainly the economy derives no benefit therefrom. On the contrary, its retention proves a clog and detriment, since it promotes excrescence. It is probably irritative, hence the sooner it is disposed of the better, since removal checks or forestalls more grave disturbances that are sure to supervene.

If protruded by inflammatory processes, it is *prima facie* evidence of danger lurking behind, possibly due to accumulated purulent fluid as in two of the cases before cited;† and in such instance common, as well as surgical, sense dictates that an outlet should be afforded the pus, no matter if it is necessary to sacrifice a portion of the cerebral substance. As already shown, the brain is exceedingly tolerant of the knife, though, that it is imperatively demanded in all cases, is by no means certain, since aspiration might fully meet the indications.‡

Exuberant granulations are repressed by the free employment of the scalpel or caustics, in accordance with indications that demand their use, in other parts of the body; and it is a logical sequence that such should not be ignored or neglected merely because the tissue is cerebral, as all evidences prove the drain of sloughing is much less taxing to the economy at large than when derived from muscular substance. Further, the claim sometimes made that caustics are dangerous because of the greater absorbent power of the brain, is not at all tenable, and is not borne

* Ashurst, "International Cyclopædia of Surgery."

† Nos. 8 and 9.

‡ Drs. Fenger and Lee, of Chicago, report aspirating a cerebral abscess by means of a hypodermic syringe, with the happiest results (1887).

out by either facts or experience. A twenty per cent. solution of chromic acid, applied almost daily for a fortnight to granular cerebral substance, in case No. 7, failed to produce the slightest toxic effect. It is *meningeal* irritation and inflammation (not of brain-substance proper) that is to be feared and guarded against, and that is so productive of mischief in this class of injuries!

Again, cerebral hernia is never an immediate or early complication, as must needs be the case if the claims of Nancrede *et al.* were tenable. Instead, it is *invariably a secondary phenomenon attendant, commonly, upon suppurative and reparative processes.* All support has frequently been removed from considerable areas of healthy brain,* and without the slightest indication of closing, falling, sinking, protrusion, or other movement of the injured surfaces; and, in the case of the canine before alluded to,† above all others, there should have been collapse and protrusion of the cerebral walls, whereas the brain and wound retained their original forms and contours, save as modified by granulations, up to the period of convalescence. The missing portion of the brain was reproduced exactly in the same manner as in muscular tissue.

When hernia is found protruding from small openings, it affords indubitable evidence of considerable force in the rear, and consequently of compression, as in the case of the young man wounded by a .22 calibre bullet;‡ and this force is dependent upon powerful effort of Nature to free the brain from impending and fatal pressure, dependent upon accumulations of inflammatory, and consequently effete, products. The merest tyro in surgery does not need to be told that the compress would but illy compensate for the knife in furuncle, paronychia, or other abscess where pus is already patent to the eye and touch, and which embody none of the dangers that accrue to brain compression. Yet, in the face of these facts, and without any attempt to classify causes, we are told that upon accumulation of pus within cerebral substance (invariably the most severe and fatal of all forms of abscess, without the slightest chance for relief by absorption), the knife must ever be avoided, and compression instituted instead.

Further, hesitation or doubt as to the propriety of employing either the knife, aspirator, ligature, or cautery, as circumstances indicate, in each individual case of cerebral hernia, to my mind must be deemed reprehensible, to say the least; and, in the light of experience, I would have as little hesitation in excising or puncturing such hernia at its very outset, as in evacuating a felon or boil, or removing a wart. Witness the case of a girl, 13 years of age, wounded in the forehead by the discharge of a gun, as reported in the *Journal of the American Medical Association* for February, 1887, and the happy result that followed ligation and strangulation of a cerebral hernia;§ or consult the "Medical and Surgical History of the Civil War," where, in not a single instance (and several cases are cited by a surgeon, who appears to have been wise beyond his time) was there an untoward result.

In conclusion, let me remark that the experiences resultant upon intradural inoculation of lower vertebrates, in connection with the microbe theory and Pasteur craze, has direct bearing, and has proved of no little value, in the study and treatment of cranial wounds and cerebral injuries, and fully corroborate the assertion of Dr. Roberts, of Philadelphia, that trephining is attended with but trifling risk if carefully performed under antiseptic precautions,—"Less than the amputation of a digit or removal of a metacarpal bone." The comparative result of fatality is but *three* per cent. in the former against *four and one-half* per cent. accruing to minor operations, while the benefits afforded by the trephine are immeasurably overwhelming. All depressed fractures, all spiculæ of bone, all bullets or other foreign substance within or upon the brain, are perpetual menaces to life or reason so long as they remain. *Per contra*, the trephine has permanently relieved epilepsy and chorea of long-standing,—in a ratio of fifty per cent. as claimed. Insanity has fled before it, and the removal of extravasated blood, fibrinous clots, accumulations of pus, tumors of various forms and characters, and irritation and ultimately fatal compression avoided. Cerebral localization, supplemented by antiseptics and drainage-tubes, has opened up a new era in brain surgery and surgical pathology, and is the key that must eventually unlock the mysteries now attendant upon diseases of the central nervous system along with their attendant reflex phenomena, and banish from the pages of surgical science the last of many lingering superstitions that obtain regarding life and mind.

FEBRUARY 15, 1889.

* Cases 2, 4, 6, and 7.

† Case 7.

‡ Case 9.

§ Vol. viii. p. 150.

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Leading Articles.

ANOTHER STEP TOWARDS THE PAUPERIZATION OF THE PUBLIC AND THE IMPOVERISHMENT OF THE PROFESSION.

DR. HENRY J. BIGELOW, of Boston, is certainly a widely-known surgeon of great ability and an able writer, and we suppose that in New England he has large influence. It is, therefore, a matter to be deeply regretted that his talents should be directed to the support of measures which certainly damage the general welfare of the profession. According to our thinking, the views which he advocates in the *Boston Medical and Surgical Journal* for April 18, although upheld by him with much plausibility, are not only incorrect, but are also very injurious to the public welfare and of sufficient importance to the general profession to require their public repudiation by every one who is in any way to be considered a representative of the profession. The question which is discussed by Dr. Bigelow is as to whether under any circumstances a member of the medical staff of a public hospital should be allowed to charge fees for services; and he takes the broad ground that no such permission should be granted, even in the most extreme cases. His argument is, we think, fairly represented in the following *résumé:*

First.—The hospital is a charity, its funds having been given for charitable purposes, and its board of managers giving their time and talents to its administration without recompense: the record of a charity should always be kept without spot, and, as it is improper that a charity should be used for the aggrandizement, or for the pecuniary benefit of any one connected with it, the medical staff should not be allowed to charge for their services.

Second.—The hospital physician is already sufficiently paid for his services by the *éclat* which his hospital position gives him, by the experience which he acquires, and by various indirect recompenses which he receives, as is shown by the fact (to quote the words of Dr. Bigelow) "that these hospital offices would command a considerable premium in money from the best class of practitioners were they annually put up at auction."

After carefully reading the article by Dr. Bigelow, we do not think that these arguments gain force by his expansion of them. Their bearing is so plain that the baldest statement of them is, to our thinking, the most forcible. They are, however, of much less force than they seem, and are very easily answered, and we shall endeavor to put the answer to them successively as baldly as we have stated them. Such answer is as follows:

First.—The hospital is in its origin, and in its general scope, a charity; but it is not a charity to all of the patients that receive its aid (unless, indeed, the charity be at the expense of the medical staff). If a patient enter the wards of the hospital and receives gratuitous care, to him the hospital is a charity. If a patient pay a portion of the expense incurred for his care, still the hospital is to him a charity. If, however, the patient pays to the hospital more than the cost of his care, including the interest upon the original plant, then the hospital is no longer to him a charity; and, if it be tarnishing its record by allowing its staff under such circumstances to charge the patient, it is equally tarnishing its record as a charity by charging such patient more than it expends on him. Take the University Hospital of Philadelphia as an example. The actual cost of the patients of the hospital, inclusive of the interest for plant, is about seven dollars a week. It charges some of its patients twenty-five dollars a week. It is ab-

surd to claim that such patient receives the dole of charity, or that the institution is to him a charitable one. So far as he is concerned, it is a commercial enterprise, selling to him certain wares, so to speak, and receiving for them a good price.

Second.—It is true that the hospital physician receives great benefit from his position, and great indirect recompense; but this benefit—his experience, his reward—comes not from the rich patient, who takes a private room in the hospital, but from the patients who are in the wards of the hospital and who, receiving from the hospital charity, are entitled to demand gratuitous service from its staff. It must also not be forgotten that the rich man in the private room usually makes a much greater draft upon the time of the doctor than does the ward patient.

We are rather surprised that a man of Dr. Bigelow's position would take the ground because in his community by pitting doctor against doctor it is possible to obtain services free, therefore it is right to do so. The frightful competition in the profession leads to very bad results, and in a comparatively small community like Boston, with perhaps one or two large hospitals offering valuable positions, probably the best men will do anything to get the positions; but in our large cities the hospitals are becoming so numerous that there is sufficient competition for leading physicians and surgeons to make them treat members of the profession with some consideration.

To put the whole matter in a nutshell,—the hospital is a charity to its charity patients, and the hospital surgeon is in honor bound to treat such patients without charge; but the rich, who seek aid at the hospital, who pay to the hospital more than they cost it, are not charity patients, and it is an injustice to require the medical officers to treat them as such, even though the hospital has the power to compel such attention.

The hospital physicians and surgeons are, however, comparatively few, and they are usually sufficiently well taken care of, so that there is no reason why the general practitioner should weep over the wrongs which are done them; but the havoc wrought by hospitals, conducted as Dr. Bigelow would like them, would be with the rank and file of the profession.

To quote Dr. Bigelow's words:

"It should not be forgotten that the outside profession must be considered. When the advantages of a hospital, intended by the public as a trust for the poor, are so organized as to divert well-to-do patients from the outside profession who are not allowed to attend them there, they have a clear right to be dissatisfied:" now it is precisely such diversion of patients that Dr. Bigelow is striving for.

Human nature desires usually to get the most it can for its money, and the great mass of well-to-do people are either not sufficiently rich or sufficiently liberal to give to a hospital or a hospital physician, or to any institution or person whom they employ, more than is demanded. The ordinary fee for a certain operation performed by the practitioner of a small inland town, say Roxbury, may be one hundred dollars; the same operation performed by the hospital medical magnate would cost two hundred dollars. The Roxburian, suffering from the malady, calculates: "I will have to pay fifteen dollars a week for four weeks to the trained nurse, and one hundred dollars to my physician, so that this whole thing at home will cost me one hundred and sixty dollars; if I go to Boston I will have to pay two hundred dollars for the operation, fifteen dollars a week for my nurse, and thirty dollars a week at least for board at a private hospital or boarding-house, making in all three hundred and eighty dollars. I will, therefore, stay at home and be operated upon." So publicly he affirms, "My own doctor is good enough for me." But the General Hospital—perhaps the Massachusetts General Hospital—says, "Not so, pay us the thirty dollars a week, and you shall have a private room, trained nursing, and the operation performed for nothing. In other words, pay us one hundred and twenty dollars, and you shall have your operation performed by the foremost surgeon of Boston,—shall get that which would cost you three hundred and eighty in Boston, and even at your own home one hundred and sixty." And so the Roxburian travels down to Boston, and the hospital, by defrauding its own surgeon, defrauds the general practitioner at Roxbury, and just so far injures the general profession. Ay, it does more than this; it pauperizes the rich man; it feeds that which grows by feeding,—namely, the spirit of obtaining services for nothing; it lowers the profession in the eyes of the public. And what does it gain? Ten or fifteen dollars a week towards the general fund.

Dr. Bigelow seems to think that there will be difficulty in drawing the line as to when the

hospital physician should be paid; but only an idiotic board of managers could have such difficulty. It can only be necessary to make the rule that under no circumstances should compensation be received by the surgeon from the ward patient; but that when a person applies for a private room he shall be told that he must make arrangements with the visiting physician if he desire his services,—the hospital furnishing only the house physician, the necessary attendants, medicine, and board.

To show the absurdity of Dr. Bigelow's proposition, let us take it to its logical conclusion. A public hospital is built, we will say, with fifty private rooms. To attend to the rich in these rooms would require all the time of the visiting physician or surgeon, who would therefore get nothing for his labor. That this supposititious case is not a mere phantom is shown by the fact that in the University Hospital of Philadelphia there are now twenty-five private rooms, most of them filled all of the time.

Year by year the problems growing out of the relation of charity to the profession are becoming more important, and apparently more and more incapable of a correct solution. In England, if the journals are to be believed, much of the profession is on the verge of bankruptcy. The same mail which brought to us Dr. Bigelow's contribution brought the April number of the *Provincial Medical Journal*, containing an editorial upon the subject. Not long since another English journal, *The Hospital*, affirmed that by the introduction of the trained chemists and the trained nurses the demand for doctors has been greatly lessened. It says that "in acute and dangerous cases the morning, and in some stances the evening and mid-day, call will be needed as before. But in lingering and chronic illnesses, where the trained nurse is always on the spot to watch and to report, and to send a swift messenger in the event of need, the daily visit will give place to the biweekly, or even to the less frequent call. Of course when visits are fewer in number fees will necessarily rise, but they can never rise so high as to compensate for the more frequent calls of former days. The doctor's income, already far too small, must become still smaller, and his struggle for survival still more severe. In due time, however, the reward will come, and the public as well as the medical profession will share in its advantages. As frequent calls upon patients become less indispensable, and fees rise, the doctor will be able to live without such a tremendous strain and expenditure of energy as he is now compelled to endure. He will have both leisure and strength to do that without which the ablest man in the world cannot keep pace with the best progress of the times. He will be able to read and to study, and, what is as necessary as either, to meet his professional brethren in frequent and friendly conference and discussion."

This decrease in the demand for physicians must continually be more and more felt, and, as *The Hospital* states, there seems to be in England "but one result, possibly starvation for many and keen competition for all." This is the goal towards which such thinkers as Dr. Bigelow would lead the profession. Surely the *Provincial Medical Journal* is right in stating that "hospitalism, if it has to land the profession into such a state as above described, should be at once suppressed in the interests not of the profession, but of society at large."

In America we are fast travelling the road towards destruction, and, with the aid of Dr. Bigelow and his coadjutors, free medical services will before long be provided for all grades of society.

At the bottom are the ordinary dispensaries; a grade above these are dispensaries connected with the teaching institutions. The way in which the latter are carried on is well shown by an occurrence which took place at the University Dispensary some time since. *First act.* Enter, a well-dressed lady. "Good-morning, doctor. Why, I did not know you were here; I owe you a bill, and I might as well pay it now." *Second act.* Payment of the bill for his services, and further attention at the dispensary to the said lady by her physician without cost.

Next to the free dispensaries comes the newly-introduced pest, the Provident Dispensaries,—the medical bucket-shops,—which are just making their appearance in our Atlantic cities, apparently like the British sparrow,—bound to become domesticated and to be a great nuisance. In these the patient pays a small sum each visit, twenty-five cents, and thereby saves his or her conscience by making a pretense of paying, and defrauds some young or possibly struggling middle-aged physician of his small fee.

At the apex of the system is the hospital, as proposed by Dr. Bigelow, robbing alike the leaders and the rank and file of the medical profession. If this system continues, the time will probably come when every young doc-

tor will start his own free dispensary, and our cities will be full of medical bucket-shops,—a condition which seems to be already reached in Great Britain, as is shown by the following extract from an editorial, which we have ventured to take at length from a recent English medical journal:

"In a conversation with a very intelligent practitioner, he raised the following defence for shilling dispensaries: 'The whole evil,' he said, 'arises from the fact that a certain number of medical men are willing to give their services gratuitously. They carry on two kinds of practice,—receiving fees at their residences, and prescribing for nothing, not for the poor, but for all classes, at their hospital consulting-rooms. One system helping the other, the hospital appointment being really a stepping-stone to success. The hospitals are ruining the profession; for the authorities of institutions getting, what is the prime factor, medical advice for nothing, are utterly regardless of the general welfare of the profession at large; and, trading as they do, are careless as to whether the recipients of this free medical relief are deserving of it or not. . . .

"Humanitarianism and so-called philanthropy are largely responsible for the present position of affairs, whilst the teachers and lecturers, who tell students that 'they should be consoled for failure by the consciousness of the nature of their work,' have also a good deal to answer for. From successful men, who have raked in their pile, this advice savors too much of humbug to be swallowed except by Verdant Greens. What can young men do? They see the old and experienced at hospitals greedily appropriating the patients who might fall into their hands. They cannot compete on equal terms, or obtain adequate fees, so they meet the difficulty in this extraordinary competition by opening shilling dispensaries, to which people flock, in large numbers, and they make a living, where otherwise they would be compelled to starve.

"It is a matter of common sense. Young men are intelligent, and can now see that all this high talk about the nobility of the profession is arrant nonsense. Men must live and pay their way. They cannot exist under any other conditions but those which regulate all other professions; and finding the means in the shilling dispensaries, they adopt this system. How will it end? It will end by the establishment of all kinds of these institutions,—some for eye diseases and surgery, some for orthopædics and skin diseases; and in turn, the profession will be brought to such a pitch that the evil will cure itself, but by the very severe process of extinguishing the whole hospital system, which for over a hundred years has been like an incubus on the profession. . . .

"You want reform, but you must be fair, and not press on those who are compelled *through evils, and not of their own creating*, to take up this line of practice. You have my views, as you have asked for them. One other point. Ask the commercial travellers who go round about their customers. They will tell you that they receive the largest orders from the owners of private dispensaries, whose waiting-rooms are crowded, and they also are good payers. In large towns, like Liverpool, the provident dispensary is a most lucrative system of practice. You don't agree with the system? Well, suggest a remedy. You will find that you will have a difficult task, unless you begin at the root of the evil,—the hospitals."

TREATMENT OF FOREIGN BODIES IN THE STOMACH.

A METHOD of treatment for foreign bodies in the stomach, which appears to be generally known and practised with almost uniform success in both England and the continent, consists in the administration simply of large amounts of potatoes, to which the diet should be restricted. It is stated by Professor Cameron, of Glasgow, that this plan, which, so far as we know, is almost unknown in this country, originated with the London pickpockets, whose custom it is to immediately swallow small articles of jewelry acquired in the pursuit of their profession, and then depend on their recovery through the evacuation which follows the abundant use of the potato diet. Several cases are on record where this method has proved eminently successful. Thus, Dr. Salzer (*Deutsche Medizinal Zeitung* for January 24, 1889) reports the case of a child who had swallowed a brass weight of three hundred grains in September, 1887, and in whom the physician was on the point of performing gastrotomy. According to Dr. Salzer's advice the child was put in bed, kept on his right side, so as to facilitate the passage through the pylorus, and then fed with as much potato, prepared in different methods to stimulate the appetite, as he could be persuaded to take. In five days the foreign body was evacuated in the fæces. He also

refers to a case of a patient who had swallowed a set of artificial teeth, and another who had swallowed a breast-pin one and a half inches in diameter, in both of which cases the foreign bodies were removed without difficulty.

At the meeting of the Society of Physicians in Vienna, at which the above cases were reported, the discussion which they stimulated led to the report of several other cases, one especially, by Hochenegg, which is especially remarkable in that it dealt with the case of a young carpenter, who, in 1884, swallowed a long nail, which was removed by gastrotomy. Two years later the patient was so unfortunate as to swallow a second nail similar in all respects to the first. The potato-cure was employed, and the nail was secured after nine days. In the *Deutsche Medizinal Zeitung* for March 11, 1889, Dr. Deichmüller refers to a case of a young girl, 10 years of age, who had accidentally swallowed a pin. Pain was complained of under the breast-bone, and Dr. Deichmüller, acting on the suggestion acquired through the report of the above cases, restricted the patient to the potato diet. Very shortly afterwards the pain disappeared from the chest and was felt in the stomach. Six days later it appeared in the right inguinal region ; two days subsequently, having increased in severity, it was felt in the left inguinal region, while in the evening of this day the foreign body was evacuated with the fæces.

It is hardly necessary for us to call attention to the principles upon which this method is based. Potatoes, as is well known, are composed of nearly twenty per cent. of carbohydrates, eighty per cent. of the solids being starch and cellulose. On account of this large amount of carbohydrate, a great portion will resist the action of the digestive juices. The cellulose and other carbohydrates increasing greatly in volume from imbibition with water lead to an accumulation of an immense amount of indigestible residue ; consequently the intestinal tube is, throughout the entire time of the administration of this food, filled with large masses of non-absorbable matter. The folds of the intestine become obliterated, and fixation of the foreign body in the intestinal tube is thus avoided. It is seen that from five to nine days, or even longer, are required for the evacuation of the foreign body, and in every case which does not seem desperate, a trial of this simple plan of treatment should precede resort to gastrotomy. In fact, at the recent meeting of the Vienna Medical College, Professor Billroth said that since the introduction of this procedure, gastrotomy for foreign bodies should become an obsolete operation.

EXALGINE—A NEW ANALGESIC.

UNDER the name of exalgine, or ortho-methylacetanilide, is described a new analgesic, a derivative of the aromatic series, which was recently discovered by Brigonnet, and which formed the subject of an interesting communication to the French Academy of Sciences by Drs. Dujardin-Beaumetz and Bardet. Exalgine (*ex*, privative, and *algos*, pain) has the formula of $C_9H_{11}NO$, and is one of the three isomeric methyl derivatives of acetanilide (*Compt.-Rend.*, cviii. 571). It occurs in needles, or in long tablet-like crystals, according as it is obtained by crystallization or from the mass after fusion. It is sparingly soluble in cold water, quite soluble in hot water, and readily soluble in very dilute alcohol, or in water to which a small amount of alcohol has been added : it fuses at 101° C. As regards its physiological action, it seems closely allied to antipyrin, although its effects upon the sensorium are more marked than the latter, while it exerts a less marked action as an antipyretic. The characteristic results are said to be obtained after an administration of a dose of from 4 to 6 grains, given at once, or from 6 to 12 grains, taken in two doses in the course of twenty-four hours, the analgesic effect being claimed to be very marked in all forms of neuralgia, including visceral neuralgia, even being superior to that of antipyrin (*Pharmaceutical Journal and Transactions*, March 30, 1889). No gastro-intestinal irritation, rash, or cyanosis has been noticed after its administration, and it, therefore, would appear to be free from the disagreeable after-effects which appear in certain cases after the use of antipyrin and acetanilide. The toxic dose is stated to be 7½ grains for every two pounds of body weight, death apparently resulting from paralysis of the respiratory centre. It is noted that after the physiological effect of the drug has been developed, while painful sensations are destroyed, the tactile sense remains unaltered.

Exalgine is eliminated by the urine, upon the quantity of which it exercises a marked effect, acting like the antipyretics of the same group, diminishing the quantity of the secretion (*National Druggist*, April 15, 1889). In

diabetes it also reduces the quantity of urine and of sugar eliminated. Like all of the derivatives of the aromatic series it is antiseptic and antithermic, as well as analgesic, though the latter property is the more marked and may be developed in doses of less than half as great as antipyrin. The following are the formulæ for its exhibition, as given by M. G. Bardet in *Les Nouveaux Remèdes* for March 24, 1889:

1. ANTINEURALGIC POTION OF EXALGINE.

℞ Exalgine, ʒi;
Kirschwasser, ʒx;
Simple sirup, ʒi;
Distilled water, sufficient to make, ℥v.

Dissolve the exalgine in the kirsch-water and add the sirup and water. The dose is from 1 to 3 tablespoonfuls in the course of the day.

2. SOLUTION OF EXALGINE.

℞ Exalgine, ʒi;
Rum, ʒx;
Distilled water, sufficient to make, ℥v.

Proceed as before. The dose is the same as above.

METHACETIN—A NEW ANTIPYRETIC.

DR. FRANZ MAHNERT, assistant to Professor von Jaksch, describes in the *Pharmaceutische Post* for April 7, 1889, the most recent addition to our rapidly-growing list of synthetic medicinal compounds. This time it is an antipyretic, and in composition might be described under the name of para-acetanisidin, its formula being

$$C_6H_4 \begin{cases} OCH_3 \\ NH.C_2H_3O \end{cases}$$

This substance is a combination of the radical acetyl with anisidine, and is, therefore, an acetyl combination of the methyl ether of para-amidophenol. From the fact that para-acetphenetidin is commonly described under the name of phenacetin, the precedent is formed of describing this compound under the name of methacetin.

This substance occurs in the form of a powder, formed of slightly reddish scales, without odor; slightly bitter to the taste; soluble in warm water, to a less degree in cold water; while it is very soluble in cold and warm alcohol. It is not more soluble in acid and alkaline liquids than when neutral; its point of fusion is 127° C. From experiments made on animals, it was seen that methacetin, as might be expected from its analogy in combination to phenacetin, possesses the power of reducing temperature,—three, four, or five degrees being lost within a few hours after its administration. Doses of 45 grains given to rabbits are markedly poisonous, producing convulsions in the posterior limbs, then in the anterior members, as in the case of poisoning with antipyrin. Its action is principally exerted on the central nervous system, death being produced in rabbits by administration of only 45 grains. In doses of from 2 to 3 grains, given to children, it exerts a marked antithermic action, the reduction of the temperature being gradually produced, remaining for several hours at the lowest point, and then gradually increasing. Frequently marked perspiration is produced by its use within an hour after its administration. If the administration of the drug corresponds to the day of remission of the fever, the thermal fall is produced more rapidly, and has a longer duration than under other circumstances. The author has employed it in three cases of pulmonary tuberculosis, one of tubercular meningitis, and one of pneumonia, the patients in all the cases being between 6 and 13 years of age. In one instance it produced slight collapse, but never vomiting, ringing in the ears, vertigo, or erythema. The urine during the administration of this drug reduces the potasso-cupric test for sugar, but does not itself contain sugar, and there is no hæmoglobinuria. Methacetin, in one per cent. solution, prevents coagulation in milk and ferment. It seems worthy of further study.

THE REVISION OF THE PHARMACOPŒIA.

DR. ROBERT AMORY, President of the Convention of 1880 for the Revision of the United States Pharmacopœia, has, in accordance with established custom, issued an address or circular calling for the appointment of delegates to the convention to be held in 1890. He asks "the several incorporated medical societies, incorporated medical colleges, incorporated colleges of pharmacy, and incorporated pharmaceutical societies throughout the United States of America, The American Medical Association and the American Pharmaceutical Association, to elect a number of delegates, not exceeding *three*," and calls upon "the Surgeon-General of the Army, Surgeon-General of the Navy, and Surgeon-General of the Marine Hospital Service to appoint each, not exceeding *three* medical officers, to attend a General Convention for the Revision

and Publication of the Pharmacopœia of the United States of America, to assemble in the city of Washington, D.C., on the first Wednesday of May, 1890 (May 7) at twelve o'clock noon."

Dr. Amory further requests that the various bodies entitled to representation submit the existing Pharmacopœia to a careful revision, and transmit the result of their labors to the General Committee of Revision, at least three months before the meeting of the General Convention. It is also very desirable that every incorporated medical or "pharmacal" [*sic*] college, association, or society desiring to be represented in the Convention will send to him its corporate title and a list of its officers, addressed to the care of Dr. Edwin H. Brigham, Assistant Librarian of the Boston Medical Library, 19 Boylston Place, Boston, Mass., in order that he may prepare a list for publication.

We call the attention of our readers very earnestly to this matter, and sincerely hope that there will be a large and strong representation of the American medical profession at the Convention. The whole profession is, or ought to be, deeply interested in the Pharmacopœia, and we think that most of our readers will agree with us in believing that the last revision of the Pharmacopœia was not a complete success, and that this result was largely dependent upon the fact that the revision was left to so great an extent in the hands of the pharmacists.

Reports on Therapeutic Progress.

THE TREATMENT OF CERTAIN CASES OF CHRONIC URÆMIA BY MORPHINE.

At the meeting of the Medical Society of London, held April 13, 1889, Dr. Stephen Mackenzie read a paper on this subject (*British Medical Journal*, April 13, 1889). He prefaced it by depicting the suffering often witnessed in patients suffering from uræmia due to Bright's disease, and was sure any suggested treatment for the relief of the distressing symptoms would receive careful consideration and discussion, even if it ran counter to current teaching. He then narrated the following cases: Case I. was a woman, aged 38, suffering from chronic diffuse nephritis of some years' standing. There were anasarca, ascites, and breathlessness; the urine contained from one-half to two-thirds of albumen. The heart was hypertrophied, and there was double papillo-retinitis. She was treated for the renal symptoms with great benefit, and in about six weeks was free from dropsy and breathlessness, and was able to leave her bed. One evening, after unusual exertion in the afternoon, she was suddenly attacked by intense dyspnœa, with respirations fifty in the minute. The heart's action was weak, 150 to 200 per minute; she was cyanosed and covered from head to foot with clammy sweat. She was in a condition of mental excitement, with great terror of dying. She was treated with nitrite of amyl, alcohol, ammonia, and ether, without any relief, and then $\frac{1}{8}$ of a grain of morphine hydrochlorate was injected hypodermically. In a few minutes the dyspnœa became less urgent, in twenty minutes the patient was able to lie down, and on the following morning was in her usual condition. Several subsequent attacks were treated in a similar manner after the failure of other remedies. Morphine hypodermically and internally never failed to afford relief, and at no time were toxic effects induced. The patient died three months and a half after the first attack of dyspnœa of symptoms of chronic Bright's disease. Case II. was a woman, aged 28, suffering from granular kidneys of four years' standing. There was considerable anasarca, about one-third albumen in the urine, hypertrophy of the left ventricle, with mitral incompetency, and double papillo-retinitis. She suffered much from breathlessness and headache. After decided improvement for a time, she suffered from severe headache, nausea, breathlessness, and irregular action of the heart, and great sleeplessness. Chloral, bromides, nitroglycerin, and inhalations of oil of juniper were used without effect, or with little effect. Liq. morphine hydrochlor., ℥x, was then administered, with rapid alleviation of all the symptoms. It was repeated on many subsequent occasions with equal benefit, and with no drawbacks. The patient died of the combined effects of a carbuncle and the renal disease about two months after the treatment was employed. 10-minim doses of the solution of morphine always promptly relieved distressing symptoms; 5-minim doses were not sufficient. The author proceeded to point out the safety with which the morphine was given in these and other recorded cases. There was a deeply-rooted belief, possibly well founded, in the danger of opium and its alkaloids in renal disease. It was quite clear that this subject required further investigation; not only might morphine be given with safety in cases of uræmia, but with positive

advantage, as in the cases narrated. Next followed a brief consideration of the nature of uræmia, and it was concluded that: 1. Uræmia was a poisoning of the nervous system,—a toxæmia. 2. The poison was developed within the body of the patient,—autotoxæmia. References were made to the writings of Grainger Stewart, Dickinson, and Carter, and more especially to the researches of Bouchard, who had described seven toxic elements in the blood in uræmia. The principles of the treatment of uræmia were declared to be three: 1, the elimination of the poisons; 2, the counteraction of the poisons; 3, the prevention of the retention of further poisons. After discussing the first and last principles of treatment which should always be employed, he proceeded to point out that the value of morphine was to fulfil the second indication. The author next discussed the nature of uræmic dyspnœa, headache, and convulsions, and attributed the beneficial effects of morphine to its freeing the blood-vessels from the spasm induced by the poison in the blood. He referred to the writings of Loomis and his followers, and to cases recently recorded by Mr. Alfred Grace, where morphine had been administered in as large quantities as from ½ to 1 whole grain for a dose, by hypodermic injections, with the best results. He concluded by saying he thought enough evidence had been brought forward to show that morphine deserved further trial in uræmia. He did not recommend its indiscriminate use, and in the light of the asserted susceptibility of patients with disease of the kidneys to the toxic effects of opium, it would be given with eyes open to its possible danger. There could, however, be no doubt as to its immediate good in suitable cases, and it was probable more lasting good would result in cases of a curable kind.

The PRESIDENT said that morphine in uræmia was kept back as a kind of bugbear, and was always held to be contraindicated. He had met with plenty of cases in which it might have proved useful, but he had never had the courage to use it. He mentioned one case, however, in which it had been given before the diagnosis of eclampsia was made, and apparently with benefit.

DR. MACKENZIE said he was particularly anxious to hear of any cases in which untoward effects had been observed as following the effects of the drug in uræmia.

MR. BLACK asked whether there was any sugar in the urine.

DR. BEEVOR said that he knew of one case, at any rate, in which opium had had a bad effect. The patient was a man who was suffering from hæmoptysis, and it was not then known that he had Bright's disease as well. He was given about three grains of opium altogether, and in the course of twenty-four hours he died in a comatose condition. He was found to have granular kidneys. He said he supposed Dr. Mackenzie would not say that there were no cases in which morphine would do harm.

DR. FLETCHER LITTLE asked whether the author had any precise observations as to the state of the kidneys in the cases alluded to. He suggested that the effects which sometimes followed morphine might depend on the extent of degenerative changes which had taken place in those organs. He mentioned that, having had occasion to look up the text-books on the subject of the relative thickness of the cortex of the kidney, he had found serious differences in the measurements given. One authority gave three millimetres, while another gave nearly half an inch. His own measurements had led him to fix four millimetres as about the average.

DR. MACKENZIE, in reply, said it was quite clear that the spasm of the blood-vessels must depend upon abnormal contents. Dr. Beevor's case supplied exactly the sort of evidence he wished to collect. He quite believed that there was some foundation for a belief so generally received. He did not think that an advanced stage of kidney degeneration afforded the necessary explanation, since cases of uræmia were met with in which the kidney lesions were only of one or two days' standing. He expressed the hope that these cases and their history would encourage others to resort to this plan of treatment; and he was sure that they possessed in it the means of relieving suffering which was otherwise without a remedy.

THE ACCUMULATION OF THE BROMIDE OF POTASSIUM IN THE ORGANISM.

In the *Lyon Médical* for March 31, 1889, DR. MAURICE DOYON calls attention to a point in the administration of the bromide of potassium which appears to have been overlooked. It is well known that the bromide of potassium is eliminated very slowly. If a single dose of 15 grains of the bromide of potassium be given to a dog the greater part of the remedy will be eliminated within twenty-four or thirty-six hours, but a certain quantity will remain, and traces of it may be

detected in the saliva and urine for at least a month. Bromide of potassium is hence a remedy which accumulates in the organism, and when constantly administered, the quantity which is retained being gradually accumulated, may prove a serious danger to the individual. It would be interesting to determine in what organ this accumulation takes place, and the following observations published by Dr. Audry seems to mark the first step in this attempt. The case was that of a child twelve years of age who for more than a year had been under constant treatment for epileptic attacks complicated with acute mania. For more than a year bromide of potassium was administered in doses of from 55 to even 120 grains a day. In November, 1888, the patient was taken with scarlet fever, and although this disease progressed normally from the first day of the child's entrance to the hospital, the patient was attacked with epilepsy and maniacal excitement. In the intervals of the attacks the child was greatly depressed, did not speak and would scarcely eat, and remained absolutely inert in the bed. In the fear that this state of somnolence might be due to the bromide of potassium, the remedy was suppressed, but at once the convulsions increased in violence and appeared at so greatly reduced intervals that the drug had to be again given, this time administered in doses of sixty grains. After a few doses violent cough set in, which was so severe as to threaten suffocation; the temperature became greatly elevated and death occurred without the presence of any other symptoms except some râles in the lungs; there was no cutaneous eruption. At the autopsy nothing peculiar could be detected with the exception that perhaps the cerebral substance appeared a little harder than is usually the case,—and the lungs in a state of congestion; all the other organs appeared healthy. The attempt was then made to determine the presence and relative amounts of the bromide of potassium in the different organs of the body. In the brain 30 grains of bromide were found; in the liver 11 grains. It would thus appear to be warrantable to conclude from this analysis that the bromide of potassium tends to accumulate in the central nervous system and in the liver; but as to what extent the accumulation may take place before becoming dangerous cannot be stated,—and whether the great somnolence observed in this case was attributable to the accumulation of the drug, or to the effect of the remedy, is likewise unknown.

CLINICAL SIGNIFICANCE OF COLORLESS STOOLS.

At the recent meeting of the Royal Medical and Chirurgical Society a paper by Dr. T. J. Walker was read by Dr. Andrew Clark as to the "Clinical Significance of Colorless or Clay-colored Stools unaccompanied by Jaundice, their Connection with Disease of the Pancreas, and on the Part played by the Pancreas in eliminating Bile from the Intestines" (*Lancet*, March 30, 1889). After referring to the accepted views of the significance of clay-colored stools, the author gave particulars of two cases in which, during life, a persistent symptom was the absence of color in the fæces, and in which the diagnosis made of obstruction of the pancreatic duct, with a healthy condition of the bile-duct, was confirmed by the necropsy. From these cases he concluded: 1. That the formation of hydrobilirubin, the coloring-matter of the fæces, depended on the mutual reaction of the bile and pancreatic fluid, under the influences met with in the intestinal tract. 2. That in disease a deficiency of pancreatic fluid would, equally with a deficiency of bile, cause the pathological condition of colorless or clay-colored stools. 3. That, since, according to the most recent physiological researches, that portion only of the colored constituents of the bile which had been converted into hydrobilirubin was excreted in the fæces, while the unchanged bilirubin, bilifuscin, and biliverdin were absorbed, it followed that if hydrobilirubin could not be produced without the aid of the pancreas, that organ must have an important *rôle* in regulating what proportion of the bile entering the intestines should be absorbed and what thrown off in the fæces. Dr. Walker then pointed out that these conclusions received confirmation from the records of other published cases, that Claude Bernard recognized that the pancreas had a part in causing the color of the fæces, and that the state in which the bile pigments were found in the meconium of the fœtus, while the pancreatic function was in abeyance, also accorded with these conclusions. He further pointed out that the fact of the pancreas influencing the excretion of the bile in the fæces would, if accepted, reconcile the discrepancy between the clinical observation that certain drugs produced copious bilious stools, and the physiological observation that these drugs had little or no influence on the secretion of bile by the liver; and that the same fact would explain those hitherto inexplicable cases in which, with no evidence of

arrest of the bile-secreting functions of the liver or of obstruction of its ducts, the symptom of white or clay-colored stools was persistently present. In conclusion, Dr. Walker indicated the practical importance of the views he had endeavored to establish in the treatment and diagnosis of pancreatic disease and of all forms of bilious disorder.

Dr. George Harley said that the paper required serious consideration, many points in it referring to matters proved, and many to others still doubtful. He quoted several instances which seemed to him to militate against the views as to the action of the pancreas which Dr. Walker had put forward. The meconium of children was only white in those cases in which the bile-duct is occluded; and in cases where a motion half black and half white was passed at the end of an attack of jaundice, the white part would be passed when the bile-duct was blocked, and the black part when it was patent, but the pancreas would be acting in both cases. White stools ought not to be so common as they were if Dr. Walker's views were correct.

Dr. Thudichum acknowledged the instructiveness of the clinical cases, but thought the conclusions drawn were false. How was it proved that bile was eliminated from the intestine? Had bile been found in the fæces? Only a very small quantity of cholic acid had been discovered in the excrement of the dog, not one-twentieth of the whole secretion. Opium would produce colorless fæces, and the same thing occurred previous to an attack of epilepsy in a child; in these cases the bile-duct was not blocked. He had searched for hydrobilirubin in the fæces, and had not been able to find any. Human bile contained bilifuscin, and nothing else, and there was no proof that the pancreatic juice converted it into hydrobilirubin. So also the idea that calomel promoted a secretion of bile was false; calomel produced sulphide of suboxide of mercury, which colored the stools green. The blood never contained bile; it at once disappeared. The formulæ quoted in the paper had been all disproved; and according to Henle the average life of a modern physiological theory was four years; therefore no data more recent than this should be quoted as authority.

Dr. Pye-Smith saw facts in the paper which chemical criticism could not detract from. The two cases related were most instructive, and were far better interpreted by the explanation Dr. Walker had given than by any other. That complete obstruction of either of the ducts would produce white fæces was a new point. Dr. Thudichum's criticism did not touch the matter in question, for it mattered not whether the coloring-matter were hydrobilirubin or not. It might be said that there was an *a priori* improbability that pancreatic disease caused absence of the color of the fæces, because the pancreas was singularly free from disease; stone in the duct was rare, as were also abscess or cancer in the head of the gland; the occurrence of undigested fat in the fæces was likewise rare. Icterus simplex was not explained by any physiological theory, and our knowledge was so incompletely established concerning the whole matter that we could not afford to put aside any explanation attempting to account for the causation of these cases.

Mr. C. B. Keetley related a case of a man who died last year of cancer of the head of the pancreas, and who had obstruction of the pancreatic duct. The gall-bladder was enormously dilated with bile, and cholecystotomy was performed; notwithstanding this obstruction, at the necropsy the bile-duct was found quite patent. The fæces were always clay-colored, the man was jaundiced, and the urine was stained with bile throughout.

Dr. Walker, in reply, said he had dealt only with cases without jaundice, where there were pancreatic symptoms with absolute absence of liver derangement. He did not desire to dispute the question whether the brown coloring-matter in the fæces were hydrobilirubin or not, and with regard to the startling announcement that bile was never to be found in the fæces or in the blood he confessed himself confused, and regretted that he could not look on these matters with the clear head of the physiological chemist. The urine in both the cases he had related was normal in color; normal bile was found in the gall-bladder and flowed into the duodenum.

EUGENOL.

The oil of cloves has for a long time been used as a local remedy for the relief of toothache; but no scientific investigation as to the actual value of the local application of this drug has yet been reported. Liebreich and Langgaard state that the oil of cloves applied to the uninjured skin first produced reddening and then anæsthesia. Recently Dr. Leubuscher has determined the presence in oil of cloves of an active principle, which he terms eugenol. He has

made a number of experiments as to the practical value of this principle, of which the following present his more important results (*Wiener Medizinische Blätter*, No. 8, 1889).

Eugenol is a clear, dark-yellow fluid; in its chemical composition allied to the higher phenols. In water it is insoluble, but readily soluble in alcohol and ether. It has an odor like the oil of cloves, and has also been described under the name of eugenic acid. If a drop of eugenol is instilled into the conjunctival sac of a rabbit, symptoms of irritation are first produced, the secretion of tears being increased, and the conjunctiva becoming somewhat reddened. After the first few minutes the sensitiveness of the cornea utterly disappears, while the conjunctiva is greatly depressed in sensibility, although not to the same degree as the cornea, the anæsthesia lasting for from ten to fifteen minutes. The deeper parts of the eye and the ciliary body are uninfluenced. In experiments to perform iridectomy in a rabbit under the influence of anæsthesia produced by eugenol, reaction occurred at the moment at which the iris was touched; while the division of the cornea was unassociated with any expression of pain. No after-effects, with the exception of slight reddening of the conjunctiva, followed the use of eugenol; the cornea remained unclouded. Similar results followed the application of eugenol to the conjunctiva of the dog, although the symptoms of irritation were here somewhat more marked than in the rabbit. The influence of eugenol was also tested on the mucous membrane of the lips, tongue, and gum of man, and at first produced slight burning, and then considerable reduction in sensibility, lasting from five to fifteen minutes. Complete anæsthesia could not, however, be produced by this remedy. On the mucous membrane of the female genitals there was slight reduction of sensibility, produced through the use of eugenol, it being more marked on the mucous membrane of the vagina than elsewhere. Applied to the uninjured skin, neither burnings nor reddening were produced, but slight reduction of the sensibility was produced in from five to six minutes. Although these results are not very striking, the author, nevertheless, tested the practical value of eugenol, and found that the best results were obtained when it was combined with a seventy per cent. solution of lanolin; ointment applied to the skin in eczema, it reduced the severity of the itching; second, in ocular surgery, the use of eugenol is not to be recommended, since in man the instillation of a dilute solution of eugenol into the conjunctival sac produces severe reaction.

THE MODERN TREATMENT OF ACUTE INTESTINAL OBSTRUCTION.

At the meeting of the Cambridge Medical Society, held February 1, 1889, MR. MARMADUKE SHEILD, F.R.C.S., read a paper on this subject (*Lancet*, April 6, 1889).

After referring to the perils of purgative treatment, he passed on to consider modern methods, and said we still find that even with all our increased knowledge and improvements a large proportion of cases of acute intestinal obstruction perish. Diagnosis is ever uncertain. It may be said that probably an internal hernia exists, or that probably a gall-stone is impacted, and so on: but when the abdomen is opened, an unsuspected invagination is found. The fact remains that, with the exception of certain cases of invagination, no living man can with certainty diagnose the condition that really exists. The importance of searching for external hernia cannot be exaggerted. In the upper classes, in those who are obese, and in females, this precaution is especially needful. Even after examination a small rupture may not be recognized. To ascertain the presence or absence of mechanical obstruction, the medical attendant should bear in mind that there are three pathological conditions which closely simulate intestinal obstruction. The first of these is acute peritonitis, perforative or otherwise; the second, acute enteritis set up by irritating ingesta; the third, rotation of an ovarian tumor on its pedicle. The third and last condition urgently calls for abdominal section, and need not be further considered. Without dwelling upon the special symptoms of these cases, he remarked that in no one of them is there true obstruction. Fæcal matter and gas will pass if properly solicited by the aid of enemata. Copious enemata, then, are the first aids to diagnosis and treatment. In cases of acute obstruction the patient is in danger of dying from starvation. Nutrient enemata can be absorbed from the colon, and should always be employed. Meanwhile, opium should be given by the mouth, and is most useful when pain, and especially shock, are marked symptoms. The fallacious improvement in the case, which always occurs under opium, will not lull the anxieties of the judicious

practitioner, who knows that fatal strangulation is still persisting. Now if the symptoms of acute obstruction should be marked, and enemata, properly given, fail to do more than evacuate the contents of the colon, it may be concluded for certain that obstruction exists. Manipulation of the abdomen, with inversion of the patient and galvanism of the abdominal walls, may be tried. The sudden inversion of a patient on a sofa or hard bed, causing the intestines to roll towards the diaphragm, may pull out a coil of bowel from under a hand, or out of an unsuspected hernia. All these measures, however, must be done by the doctor himself early in the case, thoroughly and once for all. Now supposing all this has been practised and the symptoms still persist, an abdominal section properly conducted gives the patient a chance of recovery.

CAMPHORATED NAPHTHOL IN THE TREATMENT OF TUBERCULAR ULCERATIONS.

At the meeting of the Société de Thérapeutique, held February 27, 1889, Dr. Fernet read a short communication as to the properties of camphorated naphthol and indications for its use in the treatment of tubercular ulcerations (*Le Progrès Médical*, March 9, 1889). This substance is formed of a mixture of one part naphthol and two parts of camphor. It forms a greasy liquid readily applied to skin ulcerations and accessible mucous surfaces. Dr. Fernet recommends its use in the treatment of boils, ulcerations of the skin of the mouth, and in diphtheria. He reported numerous cases of buccal tuberculosis which though not cured were greatly ameliorated by this local treatment. In the discussion which this statement provoked several other members of the society confirmed the statements of Dr. Fernet as to the use of camphorated naphthol in such cases.

ŒDEMA AS A DIAGNOSTIC SIGN IN CARCINOMA OF THE STOMACH.

M. C. Baert, of Brussels, writing in *La Clinique* on cancer of the stomach, calls attention to the frequency with which œdema of the ankles is met with in this affection after it has lasted a few months,—a diagnostic aid which is by no means new, but is, he thinks, in danger of being too much overlooked at the present day. He gives a number of cases recently occurring in the various hospitals in Brussels in which œdema was present. In one of these cases the œdema came on as early as three months after the first symptoms of the affection made their appearance; in two other cases it was noticed after four months; but in most of the other instances it was delayed till the lapse of from six months to a year after the onset. In one case, where there was no evident cause to which to attribute the loss of appetite and the wasting complained of by the patient, Professor Carpentier, noticing some œdema of the ankle, diagnosed carcinoma of the stomach, and found his diagnosis confirmed by the appearance a month afterwards of all the usual signs of the affection. Several of the cases presented a marked increase in the nitrogen excreted in the urine. With regard to the deficiency or absence of hydrochloric acid in the stomach in cancer of that organ, M. Baert admits that it is usual, but agrees with Wolff and Ewald in saying that this sign is by no means peculiar to cancer, as it is found in other gastric affections.—*Lancet*, April 13, 1889.

THE PHYSIOLOGICAL AND CLINICAL ACTION OF SULPHATE OF SPARTEINE.

In the *Deutsche Archiv für Klinische Medizin* for March 14, 1889, Dr. L. Anton Gluzinski publishes an experimental and clinical essay as to the physiological action and therapeutical value of the sulphate of sparteine. His experiments show that sparteine owes its physiological properties principally to its action on the circulatory organs, this action being more accentuated in cold-blooded animals than in mammals, and more evident after direct application to the heart-muscle than after intravenous or subcutaneous injection.

The chief symptoms of its physiological action are the retardation of the pulse, with, in a less degree, an increase of blood-pressure. In mammals, with increasing dose, the action may be divided into three stages,—first, that of slowing of the pulse; second, less marked, an increase of the pulse; and third, a final reduction of the activity of the heart. These symptoms are explained as due to the action of the drug on the pneumogastric nerve and heart-muscle, the pneumogastrics being first stimulated, while later their irritability becomes decreased. Under the influence of sparteine the muscles lose their irritability, the latent period being prolonged; while the contraction increases in energy and the wave of contraction becomes longer; in the latter stage both energy and irritability of the mus-

cles become decreased. Through the action of sparteine the inhibitory centres of the heart are paralyzed, although sparteine is without influence on the peripheral motor nerves.

The influence on the spinal cord is manifested first by increase and then by decrease of its reflex irritability, while death is due to asphyxia, produced through action on the medulla oblongata, and paralysis of all the muscles of respiration.

In applying these results to the therapeutic use of sparteine, it is evident that only the first stage is of real value; in this stage the stimulation of the peripheral ends of the pneumogastric leads to the reduction in the frequency of the heart's action, with an increase of the blood-pressure, probably attributable to an increased energy of contraction of the heart-muscle. From comparative experiments made on dogs as to the relationship between the amount of the drug and the body weight, the author concludes that the dose for man, to produce this first stage of its physiological action, should amount to not more than 1 grain at most. Experiments made in the clinic of Professor Korczynski, on the other hand, do not entirely confirm this theoretical conclusion, since Dr. Korczynski found that small doses of ⅓ to ¾ of a grain produced a more favorable action on disturbed compensation of the heart than larger doses. He gives the results of his conclusions, made in this connection, as follows:

First, sulphate of sparteine undoubtedly exerts a favorable influence on non-compensated heart lesions; *second*, the action is rapidly evinced after the administration of sparteine in its action on the pulse and the improvement of the subjective symptoms; *third*, the rapidity of action of sparteine is its distinguishing characteristic, since it is much weaker in its effects than digitalis, and, in fact, in that regard is not to be compared with it.

As regards the therapeutic indications for the use of sparteine, its rapid action is the property which renders its use most valuable, and Dr. Korczynski would, therefore, recommend the employment of sparteine in the following cases: *First*, when dangerous or severe subjective symptoms in cases of non-compensated heart-disease it is generally preferred to waiting for the slow action of digitalis; sparteine may then be combined with digitalis to accelerate the production of the desired result; *second*, when digitalis is without action or when its administration is contraindicated on any grounds; *third*, the experiment seems rational as to whether the rapid action of sparteine might not produce a favorable result in angina pectoris.

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SYPHILITIC FEVER RESEMBLING TERTIAN AGUE.

At the meeting of the Medical Society of London, held on April 1, 1889, a paper was read by Dr. Sidney Phillips on a case of syphilitic fever resembling tertian ague (*Lancet*, April 13, 1889). A woman, aged 27, a cabman's wife, with a previous healthy history, married in 1879; six months later her hair commenced to fall out; she had ulceration of the tongue and sore throat. Her first three pregnancies ended in miscarriages; she then bore four healthy children, and these were followed by another miscarriage. One of the children had died of whooping-cough, the others remained well. The husband had had syphilis six months before marriage, but the wife had not shown evidence of primary sore. In May, 1888, she was attacked with fever, accompanied by shivering and sweating, the attacks at first recurred every day, and then on alternate days. Ten days after her admission to hospital quinine was administered in 2-grain doses three times a day, and then 5 grains were given before the expected pyrexial attack. This at first checked the height of the temperature curve, but afterwards lost its effect, and an increased dose did not improve matters. At length iodide of potassium and mercury were given, which not only reduced the temperature, but also relieved the distressing headache and vomiting which had been present. The differential diagnosis from Hodgkin's disease, typhoid fever, tuberculosis, ulcerative endocarditis, pyæmia, and malaria were discussed. The question whether the pyrexia was associated with a local syphilitic lesion was raised, but no localizing evidence could be obtained. Erythema nodosum was present, and apart from syphilis it was rare to find this accompanied by fever. John Hunter and Fournier had described cases of syphilitic fever similar to that brought forward, but none so late as the ninth year after infection. These rare varieties of specific tertian fever occurred usually in females; the long duration of the pyrexia (eight months) was also a matter of interest.

THE ACTION OF FLUORIDE OF SODIUM.

In the *Archiv für Experimentelle Pathologie*, February 21, 1889, Dr. H. Tappeiner publishes an experimental essay as to the physiological action of fluoride of sodium, of which the following facts represent the more important conclusions. After doses of 7½ grains administered internally, and 2 grains given subcutaneously, for every two pounds of body weight, the following effects are produced.

First.—A condition of stupor and depression which is to be regarded principally as the result of the paralysis of the vaso-motor centres.

Second.—Convulsions which sometimes affect one limb and at other times the entire body, and in certain animals possess a distinctly epileptiform character. Occasionally they are so marked as to form the principal symptom of toxic action. In other cases, especially after the drug has been administered internally, they are but slightly marked. The convulsions are not reflex in character nor are they dependent upon the disturbance of the circulation or respiration, but are of spinal origin.

Third.—Paralysis of the vaso-motor centre.

Fourth.—Acceleration and deepening of the respiratory movements with subsequent paralysis.

Fifth.—Vomiting, and increased flow of saliva and tears, which may be arrested through the use of atropine.

THE TREATMENT OF TUBERCULOSIS IN AND NEAR JOINTS.

At the recent meeting of the Surgical Section of the Royal Academy of Medicine in Ireland, Mr. McArdle communicated the results of operative treatment in forty-one cases of local tuberculosis (*Lancet*, March 30, 1889). Of these, ten were instances of tendo-synovitis, twenty were examples of intra-osseous disease, and the remainder were cases of peri-articular and circumscribed articular tuberculosis. Of the forty-one cases, complete and permanent recovery took place in thirty-seven. In one case of tendo-synovitis, the first operation was followed by amputation; in one of trephining for central osteitis of the lower end of the tibia, the operation did not avail, as the articular cartilage was already perforated, and resection became necessary; in another of tubercular disease at the ankle, Syme's amputation became necessary; and in a case of partial arthrectomy of the knee, resection became necessary six months later, owing to the development and extension of a caseous centre in the inner condyle of the femur. In calling attention to some cases of arthrotomy, he said the following rules should guide us in performing this operation: 1. The incision should be made in such a position that arthrectomy, or even resection, can be conducted through it if necessary. 2. The incision should be sufficiently free to allow of complete removal of the diseased structures. 3. The incision should be in a position which will allow complete drainage. Neglect of any of these precautions may lead to much delay in complete removal of the diseased structures, generalization of tubercle, or sepsis.

URAL—A NEW HYPNOTIC.

Dr. Gustavo Poppi describes a product under the name of ural, formed by the combination of chloral hydrate with urethan, and recommends it as an excellent hypnotic which in ordinary doses produces a physiologically quiet sleep more rapidly and of longer duration than any of the other known hypnotics. He claims that its use produces no disagreeble accidents and in therapeutic doses no disturbance of blood pressure; if, however, poisonous doses are given to dogs under these circumstances reduction of blood-pressure is the principal mode through which its toxic action is expressed. No discomfort or distress is porduced on awakening from the sleep produced by ural, and Dr. Poppi states that its use may be persisted in for several days without inconvenience. He claims that he has given it in different cases of heart-disease, hysteria, and other nervous affections, in all cases with the best results, even when other hypnotics had proved ineffective.—*Wiener Medizinische Wochenschrift*, No. 22, 1889.

FURUNCULUS OF THE EAR.

According to the Paris correspondent of the *Lancet*, April 6, 1889, in an interesting work by Dr. Loewenberg, a well-known aurist, entitled "Therapeutical and Bacteriological Studies on Furunculus of the Ear," the author gives as the results of his investigations the following summary of his conclusions: 1. Furunculus is caused by a microbic infection proceeding from external media, the invasion taking place by the excretory ducts of the cutaneous follicles. 2. The successive appearance of furunculi in

the same individual proceeds from auto-contagion, which is effected by the transportation of cocci to the level of the cutaneous surface. 3. The same process may propagate the affection from one person to another or to several persons; furunculus is, therefore, contagious. 4. The entrance of microbes into the blood produces internal complications (metastatic abscesses) in certain cases of carbuncle, and even of furunculi, and often terminates in death. The author affirms that the micro-organism most frequently present in this affection was the staphylococcus albus, which was absent in one case only. The staphylococcus aureus and citreus were found equally often. In only one case were all three forms of staphylococci present together, and they were found in a certain number of unopened furunculi. Taking his stand on these researches, Dr. Loewenberg completely rejects the emollient treatment of this affection, which has hitherto been the classical one. He proposed, as long ago as 1881, to replace this treatment by another method, which is antiseptic or antibacteric, destined not only to abort the affection at its commencement but also to prevent its recurrence. This he effects by the use of saturated solutions of boric acid and alcohol, which he drops into the ear in cases of otorrhœa, and in cases of unbroken furunculi he employs a supersaturated solution of boric acid in alcohol, from which the author has never experienced any bad effects. Milder solutions, he says, are of little or no use in these cases.

Reviews.

DU LAVAGE DE LA VESSIE SANS SONDE À L'AIDE DE LA PRESSION ATMOSPHÉRIQUE; SES USAGES; SON APPLICATION AU TRAITEMENT DES CYSTITES DOULOUREUSES. Par Le Dr. J. M. Lavaux.

WASHING OUT THE BLADDER WITHOUT A CATHETER BY MEANS OF ATMOSPHERIC PRESSURE. ITS EMPLOYMENT IN THE TREATMENT OF PAINFUL CYSTITIS.

The above brochure is a description of a special instrument devised by Dr. Lavaux for injecting into the bladder various medicated fluids, and to be used in the treatment of diseases of this organ, more especially in cases of a painful nature. The apparatus consists of a reservoir, into which the medicated fluid is poured. This may be of any form, but it is essential to have it graduated, in order that the quantity of fluid injected may be noted. A rubber tube, one and a half to two metres long, and several nozzles. The special and important parts of the apparatus are the six nozzles, which are fastened to the end of the tube. They are made of metal, consisting of a tube three centimetres long, and have a lumen, measuring in the smallest one-third of a millimetre; the largest measures three millimetres; the increase in size for the other nozzles is one-third of a millimetre each. Over these nozzles is placed a conical-shaped rubber shield, which is the only part that comes in contact with the mucous membrane of the urethra. The method of using the apparatus is, first, to place the fluid in the reservoir, and, forcing out the air from the tube, the nozzle, covered with its shield, is passed into the urethra. The patient lying upon his back, the fluid is permitted to flow into the bladder. The proper height for the reservoir is about one and a half metres above the patient. As soon as the patient feels the desire to urinate, the nozzle is removed from the urethra, and he is allowed to empty his bladder; then the nozzle is again introduced, the fluid again permitted to flow into the bladder until the desire to pass water is again felt. This operation is repeated as often as is judged necessary.

The solutions vary according to the case. Usually it is recommended to begin with boracic acid, employing a saturated solution. In very obstinate cases a solution of nitrate of silver is also used, and where the pain is excessive the injection of a four per cent. solution of muriate of cocaine is first thrown into the bladder, and allowed to remain three or five minutes, when the other solutions can then be injected. The temperature of the injected fluids should be from 100° to 104° F., when placed in the reservoir.

Many cases, of different lesions of the bladder and urethra, are given by Dr. Lavaux, in which his method of treatment has been employed, and the results have been so very favorable that he believes they could not have been obtained by any other means of treatment. A few of the conclusions arrived at, from the author's experience, are,—that it is possible to wash out the bladder, by means of atmospheric pressure, without using a catheter, when his instrument is employed; that if his instructions are followed out, the operation is very simple, easily done, and absolutely harmless; that thorough antisepsis of the urethra and bladder can be obtained, thus lessening very much the risks of operations in this region; that obscure fistulæ of the

urethra may be diagnosticated; that it is the most suitable method of treatment in all cases of cystitis; that anæsthesia of the mucous membrane of the bladder is possible in a few minutes by using a solution of muriate of cocaine.

Not only is this method of treatment of diseases of the bladder considered by Dr. Lavaux to be the most satisfactory in men; but also in women he has found it much superior to any other procedure in this class of cases. The application is the same with women as with men, except that it is not necessary to place the reservoir so high above the patient, and to place the whole nozzle in the urethra. J. H. C. S.

MASSO-THERAPEUTICS, OR MASSAGE AS A MODE OF TREATMENT. By Wm. Murrell, M.D., F.R.C.P., etc. Fourth edition.
Philadelphia: P. Blakiston, Son & Co., 1889.

The author is a thorough believer in his subject. He gives us the history of massage. More important to the reader than this, he gives us the method of performing it. This chapter is excellently illustrated, as it should be, with wood-cuts that tell the reader a great deal. The different methods of massage —the *effleurage*, the *pètrissage*, the *massage à frictions*, the *tapotement*—are all thoroughly and clearly described. Direct mechanical stimulation is explained, and the mode of application of the various methods to the various portions of the body, and to the varying indications drawn from the diseased condition of the recipient are dwelt upon at length. Dr. Murrell is an advocate of dry rubbing; even lanolin is inferior, he thinks, to rubbing without any lubricant. "The rubber who rubbed a hole in his patient because he had no vaseline, had mistaken his vocation." Whether inunctions are valuable *per se* is another matter altogether, nor is the character, education, affability, patience of the masseur or masseuse a trivial matter. "We cannot take John from the stable or Biddy from the wash-tub, and in one easy lesson convert either into a safe, reliable, or efficient manipulator." "Skill in the proper performance of massage takes at least two years to acquire."

The morning, Dr. Murrell thinks, is the proper time; nor need the patients rest in bed an hour after it. "The sooner they go about their business the better." Nor should it be too long continued at one *séance*.

"The practice of massage," the author remarks, "has preceded the theory;" but he endeavors to formulate some theories of his own and other investigators. The question of the true physiological action of the process he considers an involved one. To guide us we have the observations of Gopadze, Zabludorski, and Von Mosengeil. The experiments of these observers and their results Dr. Murrell carefully studies, and proceeds to detail the methods of applying massage in various specified disorders,—paralysis and nervous affections in general, constipation,—in which he regards massage as holding out almost the only hope of permanent cure,—hemorrhoids, intestinal obstruction, profuse defecation, gall-stone,—of the relief of which he gives numerous clinical cases, — dyspepsia, corpulence,—where he considers it "of very great value,"—rheumatism and rheumatoid arthritis,—and his chapter on this is exceedingly able and suggestive, going, as he does, into the rationale of massage,—malassimilation, neurasthenia, —in regard to which he coins a new word, "Weirmitchellism," as a tribute to Dr. Mitchell's work; in fact, the list of diseases benefited by massage, according to Dr. Murrell, might be almost indefinitely extended, and, better still, he apparently proves his case, and has experience and observation of actual results to support him triumphantly. He quotes Dr. Maclean, of Netley, in support of the opinion that even acute diseases, as intermittent fever, are greatly benefited, and shows its value in cases of poisoning and its method of application. The reader, and the careful, conscientious reader alone, will understand the real value of Dr. Murrell's work and profit by it, and we can do no better than advise all except experts to begin its perusal. E. W. W.

A SYSTEM OF GYNÆCOLOGY. By American authors. Edited by Matthew D. Mann, A.M., M.D.. etc. Vol. II. Illustrated with four colored plates and three hundred and sixty-one engravings on wood.
Philadelphia: Lea Brothers & Co., 1888.

So very extensive is the scope and design of this work that we find a very minute division of topics and come into possession of articles on subjects, the bearing of which upon gynæcology we can at the first glance scarcely understand. All that could be said of the previous volume applies to this second one. It presents us with a similar array of excellent monographs upon gynæcologic topics, from the pen of writers of experience and acknowledged reputation. Among the contributors we find Drs. Battey, Baker,

Busey, Emmett, Englemann, Goodell, Gross, Jenks, Howard A. Kelly, Chas. Carroll Lee, Lusk, T. Gaillard Thomas, and many others. It would be hard, among so many excellent articles, to name any as especially brilliant, deep, or valuable; but perhaps general consent would recognize the chapter by Dr. S. W. Gross on "Tumors of the Breast;" that on "Diseases of the Bladder and Urethra," by W. H. Baker; Dr. Emmett's article on "Lacerations of the Cervix Uteri," and "The Treatment of Ovarian and of Extra-Ovarian Tumors," by Dr. Wm. Goodell. Among the articles on more limited subjects is an excellent one on "Fistulæ," by Dr. Edward W. Jenks; "Diseases of the Fallopian Tubes," by Henry A. Coe, M.D.; and the book concludes with "Displacements of the Uterus," by Geo. T. Harrison, M.D.,—a very unique article,—the only one, perhaps, extant in which, from beginning to end, though illustrated, is to be found no portrayal of a pessary of any shape.

The possessor of the "American System of Gynæcology" might—if he were oblivious of the signs of the times—reasonably hope that he had at last obtained a complete text-book on the subject. He might, if of feeble intellect, be lulled into a false security, and imagine that science and art, minuteness and elaboration, had done their best or their worst, and that he might turn his attention and his purse towards improving his library in some other branch of medicine; but, alas! with the new season will come new discoveries and new writers, new systems and new encyclopædic works, though the signs of the times do seem to point towards an arrest in gynæcological progress. When any branch of surgical art finds itself day by day drawing nearer to that point where the total extirpation of affected organs becomes more and more a necessity, one may feel some hope that the end is near, and that some other field of medicine or surgery will next hear the huntsman's horn and be trodden down by the huntsmen.

E. W. W.

ILLUSTRATIONS OF ACUTE BRIGHT'S DISEASE. By Francis Delafield, M.D. Part III. of Volume II. of "Studies in Pathological Anatomy."
New York: William Wood & Co., 1888.

In this division of his great work on pathological anatomy, Professor Delafield first gives the classifications of diseases of the kidney as adopted by various well-known authorities, and then takes up the consideration of acute Bright's disease, which he divides into acute exudative nephritis, acute degeneration of the kidney, and acute diffuse nephritis.

The first of these varieties is a simple acute inflammation of the kidney, which may or may not end in the necrosis of the renal epithelium. The second is described as acute changes in the kidney, characterized by the death of the epithelial cells of the tubules. This is the form which is especially produced by arsenic, mercury, and other poisons. The acute diffuse nephritis is said to offer the same lesions as have been described as belonging to exudative nephritis, but with two additional changes,—changes which are found in the earliest stages of the inflammation, and which give the characteristic stamp to the lesion,—first, a growth of connective tissue in the stroma; second, a growth of the capsule-cells of the Malpighian bodies.

We confess to doubting very much whether these different diseases really are distinct from one another, and are not simply different forms of the same thing. The distinctions between acute congestion and inflammation are always a distinction without a difference. In an organ like the kidney sometimes one, sometimes the other tissue, and sometimes various tissues together, may bear the brunt of a disease explosion, which explosions, after all, are of similar character. The most valuable part of the work are the numerous photogravures which adorn it. Some of these have the appearance of having been made directly from sections, but others of them are plainly not more than reproductions of drawings, of which character, indeed, probably are all the plates.

CHEMICAL LECTURE NOTES. By Peter T. Austen, Ph.D., F.C.S., Professor of General and Applied Chemistry, Rutgers College and the New Jersey State Scientific School.
New York: John Wiley & Sons.

AN ELEMENTARY TEXT-BOOK OF CHEMISTRY. By Wm. G. Mixter.
New York: John Wiley & Sons, 1889.

The author states in his preface that this book is not intended to be a text-book on chemistry, but simply a collection of notes on certain topics, which experience shows are frequently stumbling-blocks to the student. No order whatever seems to be followed in the arrangement of the topics, the subjects being grouped together in the most random manner possible, and it is evident that the entire course of lectures was delivered to very youthful classes. This, of course, is not a disadvantage, since the author's illustrations are on this account rendered very readily

comprehensible, although perhaps expressed in a somewhat verbose manner. The book is occupied with the general principles of chemistry, such as consideration of properties of molecules, laws of chemical reactions, the character of acids, bases, and salts, and other compounds, there being no consideration of the elements or specific chemical facts.

Professor Mixter's work is of a very different class from the above. This is a complete text-book of elementary chemistry, likewise designed for the beginner in chemistry, and it is thoroughly up to the times in knowing what to teach and how to teach it, and, more important still, what to omit. We hardly feel that the book requires further notice than to say that in looking through it we find nothing to criticise and much to commend.

Correspondence.

PARIS.

(From our Special Correspondent.)

How they use Sulphonal in France—Inulin in Artichoke-Heads—Coca Bad for Singers—Potassium Bromide Accumulation in the Brain—Antipyrin in Diabetes—Ethyl Cyanide is a Poison.

The subject of sulphonal was not exhausted in last month's letter, the physiological effects only having been considered. The pharmacological and therapeutical sides of the question, as viewed by Professor Mairet, of the Montpellier faculty, now require attention. His opinions deserve notice, not only because they somewhat differ from those of the American and German practitioners, but because they are supported by a considerable number of observations. The pharmacology can soon be disposed of. Sulphonal being tasteless, and insoluble in most menstrua, the powder is the best form to be administered, and it may be given suspended in almost any vehicle, from currant-jelly to plain water. As regards time, that to be preferred is two hours after the evening meal. The medicine being slow in acting, it must be thus given several hours before bedtime. The therapeutic effects were observed with insane patients, a restless class as a rule, numbering thirty-two, seven of whom were males. The product was first chemically examined by the hospital pharmacist, and found perfectly pure. With doses of less than 2 grammes (30 grains) sulphonal invariably failed, except once in the case of an epileptic girl. At the same time no after-effects were observed. With 30-grain doses, out of twelve cases, six were unimproved, two partially improved, and four only were afforded sleep. Not to give the particulars of each one of the successful cases, suffice it to say that in all sulphonal procured a six or seven hours' sleep, acted after the first dose, and, when the medicine had to be withheld, the sleep continued good for two nights after sulphonal was suppressed. The bad features were that from the second or third day the secondary effects became apparent, and kept daily increasing, so that the medicine could no longer be tolerated. The patients were affected like the healthy persons previously experimented with,—that is, they were in a state that can best be likened to the condition of a man after a long debauch. Next, 45-grain doses were administered in eleven cases, and in eight of them sulphonal acted successfully as a hypnotic. Some very restless patients thus procured deep and prolonged sleep. But the unpleasant secondary effects kept apace with the dose. During the day the patients would have vomitings, stay in a state of stupor, half-asleep and half-awake, and finally reach such a fit of depression and incoherency that the medicine could not be continued for more than three or four days. On stopping sulphonal, sleep would be good yet for a night or two, appetite would reappear, the feeling of drunkenness and irritation of the eyes decrease, and the patient become himself again. But at the same time night sleep would also be lost again. Finally, still larger quantities were tried with the three patients whom 45-grain doses had failed to quiet. As much as 60 and 75 grains were given to such refractory subjects, and this time they did get sleep, although one of them had vainly tried all the hypnotics of the Pharmacopœia and others besides,—that is, urethan, methylal, acetophenone, chloral, opium, and morphine. But to best describe their condition, after two days of the treatment they were, in the patient's own words, "thoroughly drunk." Of course the medicine had to be stopped, and of course also, one or two nights after, the patients were as restless and sleepless as ever. Dr. Mairet confesses he was becoming discouraged with sulphonal. But it was with regret he felt compelled to discard so valuable a remedy, answering the purpose as it does when all hypnotics have failed, never affecting the heart, and apparently sparing all internal organs. So, on pondering over the physiological properties of sulphonal, its slow action and cumulative effects, the idea suggested itself that perhaps a different mode of exhibiting would lead to better practical results. Since experi-

ments have demonstrated that in man—to the contrary of what occurs with animals—the hypnotic action of sulphonal is the first to become manifest, the impression gained strength that the better plan would evidently be to first administer large doses for a day or two, and then the patient being well under the influence of the medicine, to continue with smaller doses. Experiments in this direction were made with twelve patients, some of whom had been given sulphonal before in 45-, 60-, and 75-grain doses, but in whom it had to be stopped on account of its secondary effects. A typical instance was that of Bo., a patient suffering from acute fits of dementia, accompanied with sleeplessness, and the same woman already alluded to as having tried all the known hypnotics. After she had recovered from the unpleasant secondary effects of the previous doses, she was, on January 23, given 75 grains of sulphonal. The ensuing night's rest was only partial, and during the day she complained of heart-burns and stomach heaviness; the same evening—that is, on the 24th—only 15 grains were administered. Results: a sound sleep during the night and no trouble during the day; good appetite. The three following days, same dose and same effects. On the 29th, the previous night having been less good, 60 grains were administered, but for the four following days only 15 grains, after which the 60-grain dose had to be repeated. The smaller doses were next found again sufficient. The other cases, treated according to the same method, afforded similar results, and consequently need not here be related in detail. The foregoing also explains the mode of administration recommended. Some explanations, however, are not unnecessary. Professor Mairet has called attention to the fact that the expression "large dose," at the beginning, must be understood to have a relative meaning. For some patients 30 or 45 grains will suffice, and 60 or 75 will be required for others. And, again, the smaller dose of 15 grains will answer during four days only with some patients like Bo., while with others the remedy will hold good for nine days before the higher doses have to be again resorted to. Another and last remark suggested by the experiments is that patients do not seem to become habituated to sulphonal, when it is given as just indicated.

A correspondent of the GAZETTE (Dr. W. W. Lane, February number, page 96) having called attention to the therapeutic value of the garden artichoke, it may be of interest to here mention that M. L. Daniel, of the Sorbonne Botanical Laboratory, has been investigating the same plant, chemically, and found its edible head to contain large quantities of inulin. The glucoside in question has been found before in many roots,—burdock, elecampane, and Jerusalem artichoke, for instance; but M. Daniel has observed its presence in the calyx of many flowers of the compositæ family, such as centaury, thistle, artichoke, sunflower, etc. None, however, exists in the cichoraceæ. As is well known, inulin is, chemically, a variety of starch, whose chief use in vegetable economy is to afford to the growing plant reserve food in a concentrated form. When the enormous flower of the artichoke has bloomed not a trace of inulin is left. So far the glucoside appears to have not been tried in therapeutics. But the plants where it abounds are used in medicine, and garden artichoke has some reputation here in rheumatism and gouty affections. M. Daniel attributes to inulin the peculiar flavor of the artichoke, plainly apparent in the *Cirsium palustre*, the *Onopordon*, the Jerusalem artichoke tubers, and the *Sylibum marianum*, which are edible. The same inulin is most likely also the active principle, not very potent perhaps, but not inert by any means.

A singular statement respecting the effects of coca on the voice has been made by Dr. Sandras before the Society of Practical Medicine. He says all coca preparations and cocaine, used either through atomizers, or in infusions and inhalations, have a disastrous action on the voice of singers. It is a great mistake, he thinks, to go on repeating that coca sharpens and strengthens the vocal cords, since, on the contrary, it will slacken and deaden them through the paralysis of the extensor muscles, as many singers have found out to their cost. And, to better illustrate the truth of his averment, Dr. Sandras stopped speaking for a moment, and before the meeting drank a tablespoonful of coca wine, swallowing it slowly and by small portions. Now, speaking again, his voice sounded much altered for the worse. The words were husky, low, and scarcely audible. The aphony thus produced, however, is painless to the speaker, if it is not to the listeners. It is also difficult to cure speedily, although chloroform inhalations appear to act as an antidote. If, after chloroform, tincture of benzoin and Norwegian tar, dissolved in turpentine, are used in inhalations, the patient's voice will be greatly improved, and to such an extent as to exceed in pitch and volume what it was before. The statement, it must be confessed, is a strange

one; but if it is inaccurate it can easily be disproved with very simple experiments.

The therapeutics of bromide of potassium are by this time well known. Yet one obscure point remains,—namely, what becomes of it in the economy, and which organ does it select in preference? In other words, is bromide of potassium a cumulative medicine? The elimination of the chemical is known to be a slow one. It has been observed, for instance, that as long as one month after the bromide has been administered small quantities of it can be detected in the urine. The question now is, Which organ acts as a storing-place for the chemical? Dr. Maurice Doyon, of the Lyons hospitals, has taken—as he modestly says—the initial steps in this direction, hoping others will follow up and complete his investigations, he having made so far but one observation. It must be remarked, by the way, the opportunities for such are but seldom met with. The case was as follows: An epileptic and maniacal child, age 12, who had been under treatment for one year at the Charité Hospital, died rather suddenly of scarlet fever. Bromide of potassium had been previously given almost all the time, in doses of 1 or 1½ drachms a day. The medication was the only one that afforded the patient any relief, and its temporary suppression once was followed with such an increase of severe fits that it had to be resorted to again. At the autopsy the brain was found somewhat indurated, but deserving otherwise no particular notice. Lungs congested; pus in the bronchia; kidneys and other organs healthy. As the child had been torpid and sleepy, it was thought advisable to examine the brain and liver for bromide of potassium. The analysis was performed as follows: One-tenth of the organ was taken, cut to slices, dried, and ignited in a platinum capsule; the soluble portions of the ashes were precipitated with silver nitrate, in the presence of an excess of acid, and the precipitate, consisting of silver bromide and chloride, was weighed. From this weight the constant proportion of chloride known to exist in the organ was deducted, and the remainder, reckoned as silver bromide, was calculated as bromide of potassium. Incidentally, it must here be remarked, that Dr. Doyon's chemical process is open to criticism. Qualitatively, the results may be admitted, perhaps. But for a correct quantitative estimation the chloride and bromide should have been evacuated, not by difference, but separately. However it may be, on multiplying by ten the numbers calculated, the brain was found to contain, in all, 1.934 grammes, and the liver .72 gramme of bromide of potassium. As the first weighed fifteen hundred grammes and the second eight hundred, it follows that the proportion in the brain was much larger. Without attaching too much importance to an isolated and not altogether perfect observation, it is certainly striking that bromide of potassium, a brain-affecting medicine, should thus be found to accumulate in the very organ which is most acted upon when the chemical is administered.

A new use has been found for antipyrin. At an Academy of Medicine meeting Dr. Panas spoke of it as effective—temporarily—in checking the formation of sugar in diabetics. As might be expected, Dr. Germain Sée launched into a warm eulogy of his favorite remedy for all the ills that flesh is heir to. But he was not exactly endorsed by his fellow-academicians. Dr. Robin thought, although antipyrin might be a useful makeshift in given cases to suppress for a while sugar-elimination, yet it is a dangerous medicine to continue for more than eight or twelve days, as a gradually increasing albuminuria will result. And even some patients cannot stand it so long. Besides, antipyrin is not a specific for diabetes. As Dr. Worms observed, other substances—quinine, for instance—have been found quite as effective. Arsenic will even succeed better and be tolerated longer, and so will opium, valerian, and potassium bromide. At the same time antipyrin should not be lost sight of, but only for a change of medication.

Although chemical analogy leads to the conclusion that ethyl cyanide (hydrocyanic ether) ought to be poisonous, some writers have maintained it is harmless. True, all cyanic compounds are not harmful, since the ferrocyanides, for instance, are not poisonous. Consequently, each cyanogen combination should be judged on its own merits, a wise precaution, by the way, with most chemicals. Dr. L. Lapicque, on experimenting with animals, has found ethyl cyanide to be deadly to rabbits in the minimum proportion of one grain to two pounds of animal weight. The substance employed was of absolute chemical purity, and injected in the shape of a watery solution into the aural veins. The symptoms were those of cyanide-poisonings in general, with this important difference, however, that the effects are very slow, as long as two hours often passing before anything can be noticed, even when three times as much as a deadly dose has been injected. This peculiarity of the poison may have led former experimenters into error. Ethyl cyanide was found still more effective with dogs, less than 5 centi-

grammes to the kilogramme being sufficient in their case, a result not to be wondered at, considering the sensitiveness of dogs to prussic acid.

PARIS, April 19, 1889.

WIESBADEN.

(From our Special Correspondent.)

My presence at the Congress for Internal Medicine, a meeting which annually unites the leaders of German medical science, permits me to give you a short account of the discussion of one of the most important questions in therapeutics, which alone occupied two entire meetings of the Congress. The subject—that of the treatment of ileus—possesses greater interest, since at this meeting the discussion was not confined to the medical aspects of the question, but was shared in by a large number of eminent surgeons who were present. The results of the meeting are not important from the discovery or report of new methods of treating this affection, but from the fact, as we have already emphasized, that when a body such as this German Congress for Internal Medicine casts its vote in support of any therapeutic measure, especially one of such great importance, and when this vote is nearly a unanimous one, their conclusions may be regarded as embodying the generally accepted opinions of the times ; and in this instance they acquire especial weight from the fact of the union in the discussion of so many eminent surgeons, who, as is unfortunately too rarely the case, in this instance agree with the clinicians in their opinions. Space will not permit of my detailing the discussion of this subject in its entirety, and I shall, therefore, restrict myself to the salient points of the discussion.

The discussion was opened by Professors Leichtenstern, of Cologne, and Curschmann, of Hamburg, the former regarding the aspect of the question from the pathological point of view, the latter confining himself to the therapeutic side of the question. Nothnagel, von Zeimssen, Leube, Türgensen, Fürbringer, Mosler, Schede, and others shared in the discussion of the problem, in whose solution each one of them was fitted through previous work.

The characteristic feature of ileus—the arrest of the movement of the intestinal contents—is primarily produced either by some mechanical obstruction in the intestines, forming the so-called mechanical ileus, or through an arrest of the propulsive power, forming the so-called dynamic ileus. Both of these causes may either alone, or when associated with each other, produce permanent arrest of motion of the intestinal contents. In the latter case, when a partial closure of the bowel is rendered complete through loss of propulsive power, we then speak of mechanico-dynamic ileus. The most frequent cause of dynamic ileus is acute or chronic peritonitis of the most varied origin, but most usually starting as perityphlitis. In this group belong the forms of ileus produced after wounds of the abdomen, after laparotomy and ovariotomy, cases in which, after successful reduction of strangulated hernia, or in which, after removal of constriction of the intestines by laparotomy, the signs of impermeability of the intestine still persist. The common cause which characterizes all the cases belonging to this group is the paralysis of a given part of the intestinal tract in which the intestinal contents collect, and the *vis a tergo* acting against an immovable obstacle may even increase the difficulty by forming obstructive twistings and knuckles in the intestinal tube. Every obstruction, even of short duration, as a consequence of the arrest of the movement of the intestinal contents, leads to an increased activity of the muscular walls of the intestinal tract above the constriction, and, as a consequence, hypertrophy of the muscular coats of the upper part of the bowel is produced. So long as this increased power of the upper part of the bowel is able to overcome the resistance at the point of constriction functional compensation may be regarded as having taken place.

The recognition of ileus produced by intestinal closure from other similar diseases is an easy one. Much more difficult is it to determine the position of the obstruction. In by far the majority of cases we must be satisfied with establishing the fact that an intestinal constriction exists, and that it is present either in the large intestine or in the small intestine. If in the latter, we can still further conclude that it is situated in the lower or in the upper parts of the small or large intestine. Only in rare cases may we succeed in obtaining special data as to the anatomical cause of the obstruction. Without doubt this uncertainty in diagnosis must greatly interfere with the success of our therapeutic treatment. If we, however, recall to our mind what was said above in reference to the pathological aspects of this affection, a number of data pointing to rational therapeutic measures will suggest themselves.

The two extremes of cases between which all possible combinations are possible are the following: Either an unimportant obstruction, with complete arrest of the intestinal movement, or active peristaltic movement associated with unsurmountable obstruction. Further, it must be recollected that, in the case of any obstruction of the intestine, there is arrest of the fluid and gaseous contents of the intestine above the occlusion, and collapse and emptiness of the intestinal tract below this point; as a consequence of this, great tension of the intestinal tube is produced, which, in its turn, leads again to a great increase of the intestinal movements, more and more gas and fluid are brought towards the point of occlusion, so adding to the tension, and so again in turn increasing the peristaltic contraction, until finally all other intestinal functions are arrested. Through the enormous distention of the intestine, and the corresponding reactive tension of the diaphragm and the external abdominal walls, the internal abdominal pressure is evidently greatly increased, the meteoristic intestine is pressed down towards the empty parts, and the parts concerned in the occlusion so surrounded on all sides by distended bowel, as in air-cushions, that every movement is impossible. When such a state of affairs has been reached, it is almost inconceivable that such an obstruction could be removed by internal means alone. And yet the state of affairs is more favorable than one would at first suppose. Experience teaches that in scarcely no category of these cases is cure through internal treatment absolutely to be excluded, although it is self-evident that the chances of cure will vary according to the nature of the obstruction. This is unmistakably proved by cases in which one and the same individual is several times attacked by ileus, perhaps three or four attacks passing away before death finally occurs, when post-mortem examination may perhaps show different obstructions which might have been regarded as absolutely immovable. Further, in many instances, the clinical symptoms absolutely establish the nature of the obstruction, and yet cure has resulted from purely internal medication. This conclusion is further proved by statistics, which show that almost one-third of such cases will result in cure through the use of internal treatment alone. Even among surgeons but a small number regard the time as having come in which all cases of ileus are to be treated by operative measures. Who can, however, to-day decide as to where lies the boundary-line which marks the limit of power of internal medication, and the point at which the domain of the surgeon commences?

The danger in operative procedures does not lie in the possibility of septic infection, for this danger has been overcome by modern surgery. It lies mainly in the weakness of the heart, which complicates the chance of every patient whose nutrition has been greatly reduced by ileus, and is the more to be dreaded, and will the earlier appear; the longer the operation lasts, the more manipulation is required, the greater the difficulty met with in the reposition of the intestinal coils. If one has the good fortune to make an early diagnosis as regards the situation of the obstruction, and the strength of the patient is such as to enable the danger of collapse to be set to one side, in such case laparotomy may triumph. In other cases, which are not so favorable and which allow but little chance for the success of internal treatment, the abdominal cavity may be opened and the obstruction rapidly sought for. Should success not crown this search, an artificial anus should be made and the abdominal wound closed. In certain of these cases the integrity of the intestinal tube may be restored a few days after this operative interference, or, should this not be the case, it may then be possible to so improve the condition of the patient as to enable him to undergo a more radical operation; but a great number of cases, especially in hospital practice, will offer no prospect of success from surgical treatment, and must remain reserved for internal therapy. This must consist of absolute diet and the administration of opium. The treatment with cathartics must be absolutely discarded, since it only tends to increase the peristalsis, which is already so dangerous a factor in the disease. At first, when the patient's strength has not been reduced, opium may be boldly given. Only in dangerous collapse is especial care to be taken in the adjustment of the doses.

Together with the opium treatment, which reduces peristalsis and abdominal pressure, may be combined the emptying of the stomach by means of the sound; this is not only to be regarded as a palliative measure, but often also exerts a true curative action, for if the stomach is emptied the tendency is for the intestinal contents above the point of obstruction to flow towards the stomach, so relieving pressure, and so perhaps entirely relieving the

invaginated portion of the bowel. Gas-puncture of the intestine acts in a similar manner in relieving tension, and so tending to equalize the pressure above and below the point of obstruction. In carefully-selected cases and carefully performed the procedure is free from danger.

If the obstruction is located deep down, this may be relieved by copious enemata, or, still better, by the introduction of air through the rectum.

SULPHUR IN DIPHTHERIA.

To the Editors of the THERAPEUTIC GAZETTE:

GENTLEMEN:—As you are in a great measure responsible for the production of this little instrument, I ask a space in your most valuable GAZETTE to recommend it to the profession, and through them to those for whose benefit it was produced. Some articles in the THERAPEUTIC GAZETTE last March, April, and May, on the use of sulphur in diphtheria, attracted my attention, and further clinical investigation convinced me of their correctness. In casting about for a means for the local application of this agent, and being tired of quills, glass tubes, and putty-blowers, I devised this instrument, one of which I send you.

It consists of a tube of brass, with a bent spoon soldered on one end. The spoon bent on itself; the whole smooth and without joints, in which poisonous substances might collect; nickel-plated and highly polished. It is to be used by passing it over the tongue, then pressing downward and forward. (I have found the patients, especially children, object much less to this action than they do to the pressure of an ordinary spatula on the middle of the tongue.) The powder then to be blown into the throat and on the tonsils by the mouth, tube of ordinary syringe, by compressing the bulb, or any other insufflator. It has proven a most handy instrument for the physician's personal use for office or bedside, used with any powder desired.

Now, going back to the question of sulphur, I would ask, What is the use of trying all sorts of unknown and uncertain agents for the destruction of microbes, when we *know* sulphur will destroy them. And if it is said that sulphurous acid is the agent demanded, and that the heat of the body is not great enough to convert the sulphur into this agent, I simply reply, that this condition must be in the sulphur itself, else *why does it do it?* If we want to kill the itch under the skin, we rub in sulphur. If we want to kill lice on birds, or *parasites* on beasts, we powder them with sulphur. When, after many years, investigation in search of an agent that would destroy the phylloxera that was destroying the vines and prostrating the grape industry in all parts of the world, and of which, in 1874, Professor Riley, of the Smithsonian Institution, in his reports of "Insects in Missouri," says, "Many remedies have been tried, but few are available in practical experience," what was it but powdered sulphur that proved the "microbe's relentless foe, and saved the industry," the vine, and the wines?

I fully believe that if the directions of the city boards of health as to sulphur fumes, printed and easily procurable, were known and followed in every family, and some appliance, like the one I have described, were at hand, and the powder of sulphur were used early and constantly in the throat, and taken internally, we might be able to stamp out this pest that is the cause of more sorrow and dread, and that destroys more young lives than almost any disease we have to treat, the scourge of the land and the dread of the physician.

Now to the use of my instrument. To be of use in the household it must be cheap, clean, and effective. It must be handy, and made of such material that it can be washed after using with boiling water, and so cheap that no one will hesitate to buy it. This one I have described is made so as to be sold by druggists for twenty-five cents, and so light that, until they keep it (which I hope will be soon), I will see that one is forwarded to any one who will send twenty cents in stamps.

J. D. OSBORNE, M.D.
NEWARK, N. J., 1034 BROAD STREET.

THE DOSE OF THE FLUID EXTRACT OF DIGITALIS.

To the Editors of the THERAPEUTIC GAZETTE:

GENTLEMEN:—In a paper, entitled "Three Cases of Valvular Insufficiency," by Dr. Kemper, in the March number of the THERAPEUTIC GAZETTE, p. 177, occurs the following statement: "The patient remained in bed for three days, during the first two of which she took 2-grain doses of bromide of caffeine, after that 10-drop doses of fluid extract of digitalis." The fluid extracts are concentrated preparations, and the fluid extract of digitalis is no exception to the rule, 100 cubic centimetres representing the therapeutic activity of 100 grammes of powdered digitalis.

The dose is 1 to 2 minims. I find that, under ordinary circumstances, 30 minims of this fluid extract is about equivalent to 50 drops, this being the average of a number of trials with different vials of the ordinary dispensing sizes. This would make a 10-drop dose of the fluid extract of digitalis about equivalent to 6 minims, an excessive and, under some circumstances, a dangerous amount. As Dr. Kemper communicates no information in regard to the number of such doses administered, or the intervals between them, his statement is liable to mislead the unwary.

Yours respectfully,

J. C. WILSON, M.D.

April 2, 1889.

Notes and Queries.

ANTIFEBRIN AS AN EXTERNAL APPLICATION.

DR. A. H. NEWTH writes in the *Lancet* for April 6, 1889, that he has recently been combining antifebrin with lanolin or vaseline, in the proportion of twenty grains to the ounce for an external application. He finds that in obstinate irritable ulcers it soothes the pain and subdues the inflammation; while in psoriasis, combined with some mercurial preparation, its results have been most excellent. He has also tried it in erythema, erysipelas, eczema, herpes, urticaria, and other complaints associated with considerable irritation, and has found it a most useful adjunct to other suitable remedies.

THERAPEUTICS OF BISULPHIDE OF CARBON.

Some observations on the employment of bisulphide of carbon, made chiefly in the Purísima Hospital, Santiago, South America, are published in the *Revista Médica de Chile*, and show that this unpleasant remedy may have more therapeutic power than it is usually credited with. It has occasionally been employed in this country for diarrhœa, and externally for ulcers. Sapelier also employed it with apparent advantage in typhoid fever, and it has been recommended in diphtheria and other diseases in which micro-organisms occur. The Chilian cases referred to included one of acute and one of chronic dysentery, one of atonic dyspepsia, one of simple gastric ulcer, and one of typhoid fever. The dose given was generally about two ounces of a saturated solution of the bisulphide in water, mixed with half a tumbler of milk or a little sirup, taken half an hour or so before meals. This very quickly relieved the pain and tenesmus. In the typhoid case, which occurred in a child of 10 (not a hospital patient), whose condition was becoming alarming, enemas of a pint of water, containing about half a drachm of bisulphide of carbon, were given, in addition to the internal administration of iodide of potassium and kairin. The diarrhœa diminished, the stools rapidly lost their offensive character, the gurgling disappeared from the iliac region, and the patient made a good recovery.—*Lancet*, March 23, 1889.

POISONING FROM HYOSCINE.

In the *University Medical Magazine* for April, 1889, DR. WM. A. CAREY reports three cases, one occurring in his own person, in which alarming symptoms followed the use of $\frac{1}{100}$ grain of the hydrobromate of hyoscine.

TREATMENT AND CURE OF TETANUS BY PILOCARPINE.

In the *Wiener Medizinische Blätter*, No. 6, 1889, is a notice of three cases of traumatic tetanus reported by DR. LUIGI CASATI, treated by the injection of the muriate of pilocarpine, in all of which cure resulted. The dose of the drug employed is not stated, with the exception that quantities of this drug may be used in tetanus with favorable result, which in healthy individuals and animals would prove fatal.

CAUSTICITY OF CARBOLIC ACID.

Some interesting details were given recently by M. CARLES to the Bordeaux Medical Society explanatory of the varying effects produced by solutions of carbolic acid upon mucous membrane, according to the solvent used (*Bull. Soc. Pharm. Bord.*, February, p. 41). If carbolic acid be added to pure neutral glycerin, in any proportion up to any equal weight, a mixture is obtained which is tolerated for a long time on the epidermis of the hand without inconvenience, while the smaller proportions are borne even by the mucous membrane of the nose; but if the glycerin used be already aqueous, or if the mixture be diluted with water, it becomes irritant and caustic, and is no longer tolerated even by the skin. Considering that glycerin

is an alcohol, and that phenol is also nearly allied to the alcohols, M. Carles inferred that when the two were brought together in a pure state a kind of compound ether is formed that is easily decomposed by water, the constituents then regaining their characteristic properties. This was confirmed by the behavior of a saturated solution of carbolic acid in ethylic alcohol, a drop of which left upon the hand for several minutes produced no unpleasant sensation or blanching of the skin; but immediately upon moistening the alcoholic spot with water there was produced a violent smarting, followed by a persistent reddening of the skin. M. Carles, therefore, suggests that in cases of burning with concentrated carbolic acid it would be better to remove the caustic with strong alcohol than with water.—*Pharmaceutical Journal and Transactions*, March 30, 1889.

ECZEMA CAUSED BY IODOFORM.

Dr. H. Campbell Pope writes in the *Lancet* for April 6, 1889, that he recently had under his care a case of inflamed bursa near the elbow-joint, which was opened antiseptically and dressed with iodoform gauze. A violent attack of eczema ensued, spreading over the whole of the limb. The eczema was cured by oleate of zinc ointment. The bursa, having filled a second time some months afterwards, was opened and dressed as before, with a like result,—namely, an attack of eczema. On discontinuing the iodoform gauze and dressing with oleate of zinc ointment, the attack soon passed off. On a third occasion, before the wound had thoroughly healed, the scab was accidentally removed. Iodoform ointment was inadvertently prescribed, and eczema appeared for the third time. It would seem in this case that iodoform was the exciting cause on each occasion of the eczema.

THE INCOMPATIBILITY OF SALICYLATE OF SODIUM AND ANTIPYRIN.

When antipyrin and salicylate of sodium in powder are mixed together an oily body is rapidly formed, which has by the end of twenty-four hours transformed the mixture into a pasty alkaline mass, in which the decomposition increases from day to day. Consequently antipyrin and salicylate of sodium should never be united in one capsule. However, it is stated by Vigier, in the *Gazette Hebdomadaire de Médecine et de Chirurgie* for March 29, 1889, that solutions of antipyrin and salicylate of sodium may be mixed and preserved indefinitely. No investigation as to the character of the decomposition which takes place when the powders are mixed has been published, and it seems hard to reconcile the fact of this decomposition occurring in the dry state when the solutions of the substances are permanent even after mixture. The subject should be certainly further investigated.

THE REGULATION OF DOSES FOR CHILDREN.

In prescribing for children one is often embarrassed as to the adjustment to the age of the child of the dose of the remedy indicated. Ordinarily the method of Gaubius is followed, which is given in the following table:

Child less than 6 months of age	$\frac{1}{20}$
Child less than 1 year of age	$\frac{1}{15}$ to $\frac{1}{12}$
Child less than 2 years of age	$\frac{1}{8}$
Child less than 3 years of age	$\frac{1}{6}$
Child less than 4 years of age	$\frac{1}{4}$
Child less than 7 years of age	$\frac{1}{3}$
Child less than 14 years of age	$\frac{1}{2}$
Child less than 16 years of age	$\frac{2}{3}$

Young has reduced this table to a formula which is pretty generally adopted in the United States. Number of years plus 12, divided by the number of years, equals the required dose. Thus, at 2 years of age $\frac{2+12}{2} = 7$, meaning that $\frac{1}{7}$ dose should be given. It is thus seen that if the number of years plus 12 be divided by the number of years, the quantity will represent the denominator of the fraction, the numerator always being 1. According to this method the proportions of doses are as follows:

For a child of 1 year	$\frac{1}{13}$
For a child of 2 years	$\frac{1}{7}$
For a child of 3 years	$\frac{1}{5}$
For a child of 4 years	$\frac{1}{4}$
For a child of 7 years	$\frac{1}{3}$
For a child of 14 years	$\frac{1}{2}$

The opiates should be given in one-half the above doses, while purgatives may be given in double or three times the quantity. —*Revue Générale de Clinique et de Thérapeutique*, March 14, 1889.

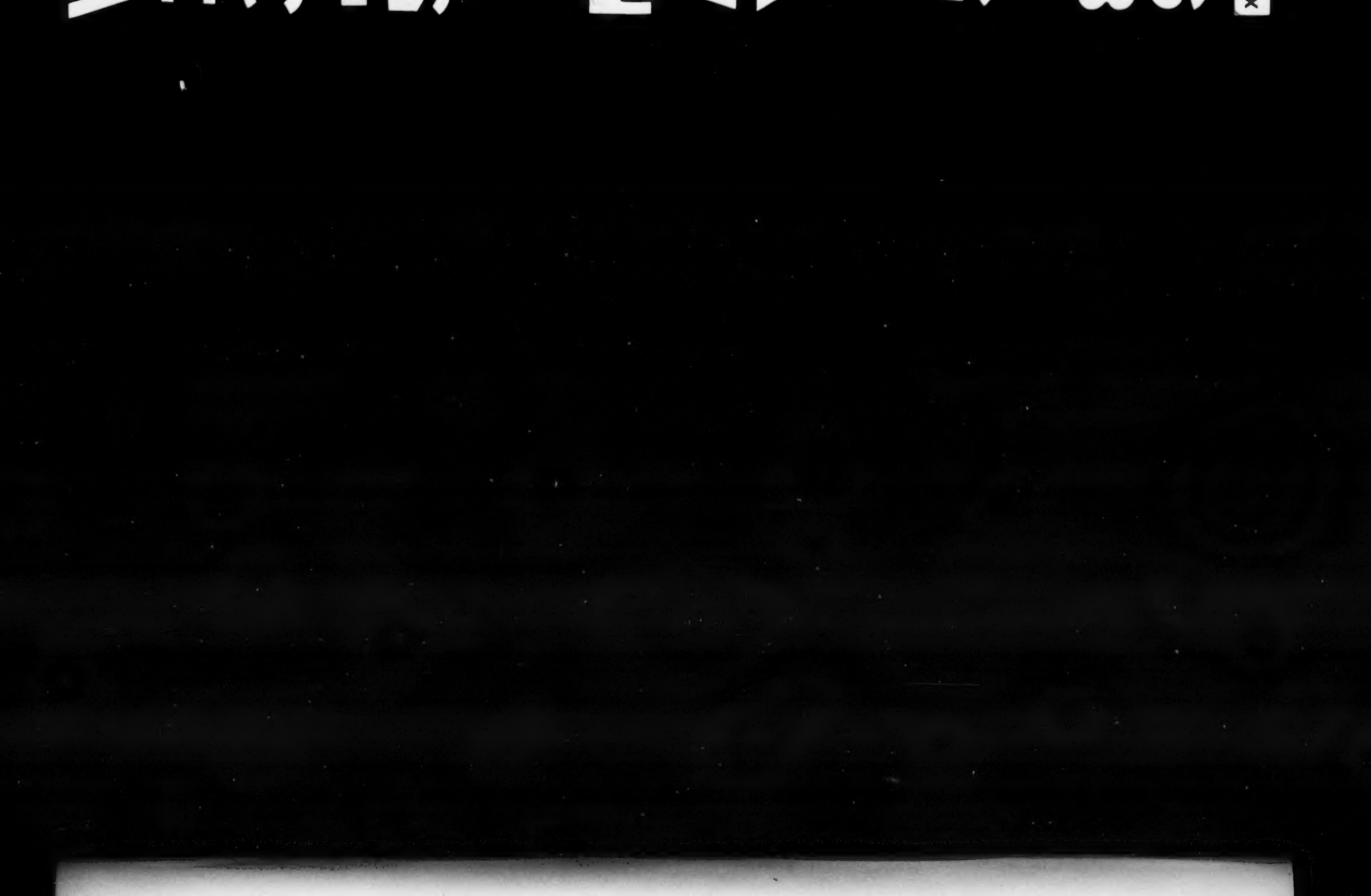
MR. STANLEY ON ARROW POISON.

The letter from Mr. H. M. Stanley, which was read at the meeting of the Royal Geographical Society on Monday last, contained

an extremely interesting reference to the arrow poison employed by the natives of the Lower Congo district, and it afforded a curious insight into the strange perversions of knowledge by which the advances of civilization are retarded. Mr. Stanley says they were much exercised as to what might be the poison on the heads of the arrows by which Lieutenant Stairs and several others were wounded, and from the effects of which four persons died almost directly. The mystery was solved by finding at Arisibba several packets of dried red ants. The bodies of these insects were dried, ground into powder, cooked in palm oil, and smeared on the points of arrows. It is well known that formic acid exists in the free state in red ants, as well as in stinging-nettles, and in several species of caterpillars. This acid is, in the pure state, so corrosive that it produces blisters on the skin, and hence there is little ground for doubting that it was the "deadly irritant by which so many men had been lost with such terrible suffering." The multitude of curious insects encountered, which rendered their lives "as miserable as they could well be," bears out Mr. Stanley's idea that many similar poisons could be prepared from insects. It certainly is strange that, with the exception of cantharides, and perhaps of blatta orientalis, the insect world is so little used for active therapeutics.—*Lancet*, April 13, 1889.

A NEW MYDRIATIC.

Some recent researches upon hydronaphthylamine, by MESSRS. BAMBERGER and MÜLLER (*Berichte*, xxi. 1112), would appear likely to result in another useful contribution from the chemical laboratory to the materia medica. The compound in question bears the systematic name tetrahydro-β-naphthylamine ($C_{10}H_7H_4NH_2$), and appears among the other interesting physiological properties to be capable of causing the dilatation of the pupil of the eye in a remarkable manner. According to Professor Filehne, of Breslau, the instillation of a little one to five per cent. solution causes the dilatation of the pupil of the eye treated only; but dilatation takes place to a strong degree in both eyes as soon as a sufficient quantity—still very minute—has become absorbed in any way, as, for instance, after subcutaneous injection. The pupils are dilated wider than by atropine, and a pupil already dilated by a maximum dose of atropine is still further dilated by this substance. The physiological action of the tetrahydronaphthylamine is not identical with that exerted by the tropeines in the production of mydriasis, since, when applied locally, instead of paralyzing the contracting muscles of the pupil, as they do, it stimulates the nerve-endings with the dilating mechanism.—*Pharmaceutical Journal and Transactions*, March 30, 1889.

A HINT FOR FACILITATING THE MICROSCOPICAL EXAMINATION OF URINE.

When attempting to examine urine under the microscope for casts, epithelial cells, and other organic bodies, a good deal of annoyance and difficulty is sometimes caused both by urates, and also, when the specimen is not quite fresh, by fermentation and putrefactive products. In order to obviate this difficulty, and with the further view of preserving the specimen, DR. M. WENDRINGER advises that the urine should be mixed with a nearly saturated solution of borax and boracic acid. This dissolves the urates and keeps the urine from fermenting, and at the same time exercises no destructive effect upon the casts and epithelial elements which it is desired to examine. The solution is prepared by mixing 12 parts of powdered borax in 100 parts of hot water, and then adding a similar quantity of boracic acid, stirring the mixture well. It is filtered while hot. On long standing, a small deposit crystallizes out, but clings to the side of the vessel, so that it does not interfere with the transparency of the liquid. The urine to be examined is put into a conical glass, and from a fifth to a third of its bulk of the boracic solution added to it and agitated with it. The urine will be found to become clear in a short time,—*i.e.*, if there is no cloudiness due to bacteria; and it will remain unchanged for several days. If it is only wanted to clear the urine and to make it keep for a day or two, the addition of a smaller quantity of the boracic solution is sufficient. If a third of its bulk is added, no fermentation or putrefactive processes take place, even if the glass is left uncovered in warm places. Albumen, too, if it exist, is not coagulated. The organic elements—as epithelial cells, casts, blood-corpuscles, etc.—collect so quickly, without undergoing any morphological change at the bottom of the glass, that the first drop taken up by the pipette usually proves a satisfactory specimen.—*Lancet*, April 6, 1889.